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TO
THE OBSERVATORY,
A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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Date.	Day of Year.	Fr. of Year.	SUN		☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	MOON'S PHASES.
			Sets.	Rises.				
1906.			h m	h m	° ′	h m s	h m s	d h m
Jan. 7	7	·016	4 6	20 6	22 27 S	0 6 11	19 4 36	Jan.
14	14	·036	4 16	20 2	21 25	9 0	19 32 12	☽ 2 2 52
21	21	·055	4 27	19 55	20 2	11 19	19 59 45	○ 10 4 37
28	28	·074	4 39	19 46	18 21	13 1	20 27 26	☾ 17 8 49
								● 24 5 9
Feb. 4	35	·093	4 52	19 35	16 24	14 3	20 54 57	Feb.
11	42	·112	5 5	19 23	14 13	14 25	21 22 23	☽ 1 0 31
18	49	·131	5 17	19 10	11 51	14 9	21 50 9	○ 8 19 46
25	56	·151	5 30	18 55	9 19	13 20	22 17 45	☾ 15 16 23
								● 22 19 57
Mar. 4	63	·170	5 42	18 40	6 41	12 2	22 45 21	March.
11	70	·189	5 54	18 24	3 57	10 20	23 12 57	☽ 2 21 28
18	77	·208	5 7	18 8	1 12 S	8 23	23 40 33	○ 10 8 17
25	84	·227	5 19	17 53	1 34 N	6 17	0 8 9	☾ 16 23 57
								● 24 11 52
April 1	91	·246	6 30	17 37	4 17	4 9	0 35 44	April.
8	98	·266	6 41	17 21	6 58	2 5	1 3 20	☽ 1 16 2
15	105	·285	6 53	17 6	9 32	0 12	1 30 56	○ 8 18 12
22	112	·304	7 5	16 51	11 59	23 58 36	1 58 32	☾ 15 8 37
29	119	·323	7 16	16 37	14 16	57 22	2 26 8	● 23 4 7
								May.
May 6	126	·342	7 28	16 24	16 21	56 33	2 53 44	☽ 1 7 7
13	133	·361	7 39	16 13	18 13	56 10	3 21 20	○ 8 2 10
20	140	·381	7 48	16 3	19 51	56 16	3 48 56	☾ 14 19 3
27	147	·400	7 59	15 55	21 11	56 50	4 16 31	● 22 20 1
								☽ 30 18 24
June 3	154	·419	8 6	15 49	22 14	57 46	4 44 7	June.
10	161	·438	8 12	15 45	22 58	59 0	5 11 43	○ 6 9 12
17	168	·457	8 16	15 44	23 22	0 0 27	5 39 19	☾ 13 7 34
24	175	·476	8 18	15 45	23 26	1 59	6 6 55	● 21 11 6
								☽ 29 2 19
July 1	182	·496	8 17	15 49	23 10	3 26	6 34 31	July.
8	189	·515	8 14	15 54	22 34	4 41	7 2 7	○ 5 16 28
15	196	·534	8 10	16 3	21 39	5 37	7 29 43	☾ 12 22 13
22	203	·553	8 2	16 11	20 25	6 4	7 57 19	● 21 0 59
29	210	·572	7 52	16 21	18 55	6 17	8 24 54	☽ 28 7 56
								August.
Aug. 5	217	·591	7 40	16 32	17 10	5 53	8 52 30	○ 4 1 0
12	224	·611	7 29	16 42	15 11	4 59	9 20 6	☾ 11 14 48
19	231	·630	7 14	16 53	13 0	3 39	9 47 42	● 19 13 28
26	238	·649	6 59	17 5	10 39	1 55	10 15 18	☽ 26 12 43
								Sept.
Sept. 2	245	·668	6 44	17 16	8 10	23 59 50	10 42 54	○ 2 11 36
9	252	·687	6 29	17 27	5 34	57 31	11 10 30	☾ 10 8 54
16	259	·706	6 13	17 38	2 54	55 5	11 38 6	● 18 0 34
23	266	·726	5 57	17 49	0 11 N	52 37	12 5 42	☽ 24 18 11
30	273	·745	5 41	18 0	2 33 S	50 15	12 33 17	Oct.
								○ 2 0 48
Oct. 7	280	·764	5 24	18 13	5 15	48 4	13 0 53	☾ 10 3 39
14	287	·783	5 9	18 24	7 54	46 13	13 28 29	● 17 10 43
21	294	·802	5 54	18 36	10 28	44 49	13 56 5	☽ 24 1 50
28	301	·821	4 40	18 48	12 55	43 56	14 23 41	○ 31 16 46
								Nov.
Nov. 4	308	·841	4 28	19 1	15 11	43 39	14 51 17	☾ 8 21 45
11	315	·860	4 16	19 13	17 15	44 3	15 18 53	● 15 20 37
18	322	·879	4 6	19 25	19 5	45 8	15 46 29	☽ 22 12 39
25	329	·898	3 58	19 37	20 38	46 53	16 14 4	○ 30 11 7
								Dec.
Dec. 2	336	·917	3 53	19 47	21 52	49 15	16 41 40	☾ 8 13 45
9	343	·936	3 50	19 56	22 46	52 9	17 9 16	● 15 6 54
16	*350*	*·956*	*3 49*	20 2	23 18	55 25	17 36 52	☽ 22 3 4
23	*357*	*·975*	*3 52*	20 6	23 27	58 53	18 4 28	○ 30 6 44
30	*364*	*·994*	*3 57*	20 8	23 13 S	0 2 21	18 32 4	

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.						FEBRUARY.						MARCH.						APRIL.					
DAY.		MOON.				DAY.		MOON.				DAY.		MOON.				DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°			h m	h m	° ′	°			h m	h m	° ′	°			h m	h m	° ′	°
1	M	11 25	5 25	1 39 S	+10·9 M.	1	Th	13 25	6 11	11 59 N	− 6·1 M.	1	Th	12 14	4 48	14 0 N	+13·2 M.	1	**S**	13 52	5 50	19 49 N	− 4·4 M.
2	Tu	12 29	6 8	2 22 N	− 1·3 M.	2	F	14 26	6 55	14 55	−18·3 M.	2	F	13 14	5 34	16 33	+ 1·0 M.	2	M	14 38	6 40	19 20	−16·6 M.
3	W	13 32	6 50	6 15	−13·5 M.	3	S	15 27	7 41	17 15	−30·4 M.	3	S	14 12	6 20	18 24	−11·1 M.	3	Tu	15 18	7 31	17 51	−28·8 M.
4	Th	14 35	7 33	9 53	−25·6 M.	4	**S**	16 26	8 29	18 51	−42·6 M.	4	**S**	15 9	7 9	19 27	−23·3 M.	4	W	15 55	8 23	15 24	−41·0 M.
5	F	15 39	8 16	13 9	−37·8 M.	5	M	17 20	9 19	19 35	−54·7 M.	5	M	16 0	7 59	19 35	−35·5 M.	5	Th	16 26	9 15	12 1	−53·2 M.
6	S	16 41	9 2	15 54	−49·9 M.	6	Tu	18 9	10 11	19 20	−66·8 M.	6	Tu	16 45	8 51	18 44	−47·6 M.	6	F	16 55	10 8	7 52	−65·3 M.
7	**S**	17 40	9 49	17 59	−62·0 M.	7	W	18 52	11 4	18 4	−79·0 M.	7	W	17 26	9 44	16 51	−59·8 M.	7	S	17 23	11 1	3 8 N	−77·5 M.
8	M	18 39	10 39	19 17	−74·2 M.	8	Th	rises	11 58	15 46	+88·9 E.	8	Th	17 58	10 37	13 58	−71·9 M.	8	**S**	rises	11 55	1 54 S	−89·6 M.
9	Tu	rises	11 30	19 39	−86·3 M.	9	F	5 27	12 50	12 32	+76·8 E.	9	F	18 30	11 30	10 13	−84·1 M.	9	M	7 10	12 50	6 53	+78·2 E.
10	W	4 22	12 22	19 1	+81·6 E.	10	S	6 41	13 43	8 34	+64·6 E.	10	S	rises	12 23	5 48	+83·8 E.	10	Tu	8 33	13 46	11 28	+66·1 E.
11	Th	5 23	13 15	17 22	+69·5 E.	11	**S**	7 58	14 35	4 4 N	+52·5 E.	11	**S**	6 54	13 16	0 58 N	+71·6 E.	11	W	9 54	14 45	15 17	+53·9 E.
12	F	6 32	14 7	14 46	+57·3 E.	12	M	9 15	15 26	0 41 S	+40·4 E.	12	M	8 15	14 10	3 58 S	+59·5 E.	12	Th	11 11	15 44	18 3	+41·7 E.
13	S	7 42	14 58	11 21	+45·2 E.	13	Tu	10 34	16 18	5 25	+28·2 E.	13	Tu	9 35	15 4	8 40	+47·3 E.	13	F	12 22	16 42	19 35	+29·5 E.
14	**S**	8 57	15 49	7 17	+33·1 E.	14	W	11 51	17 11	9 50	+16·1 E.	14	W	10 55	15 59	12 49	+35·2 E.	14	S	13 22	17 40	19 52	+17·4 E.
15	M	10 11	16 39	2 47 N	+20·9 E.	15	Th	13 9	18 5	13 40	+ 3·9 E.	15	Th	12 13	16 56	16 9	+23·0 E.	15	**S**	14 10	18 35	18 57	+ 5·2 E.
16	Tu	11 29	17 30	1 53 S	+ 8·8 E.	16	F	14 22	19 0	16 41	− 8·3 E.	16	F	13 25	17 53	18 27	+10·8 E.	16	M	14 49	19 28	17 0	− 7·0 E.
17	W	12 44	18 21	6 30	− 3·4 E.	17	S	15 31	19 57	18 42	−20·5 E.	17	S	14 29	18 49	19 35	− 1·3 E.	17	Tu	15 23	20 17	14 13	−19·3 E.
18	Th	14 3	19 14	10 47	−15·5 E.	18	**S**	16 34	20 54	19 35	−32·6 E.	18	**S**	15 23	19 45	19 33	−13·5 E.	18	W	15 49	21 4	10 49	−31·5 E.
19	F	15 18	20 9	14 29	−27·7 E.	19	M	17 25	21 49	19 17	−44·8 E.	19	M	16 8	20 39	18 23	−25·7 E.	19	Th	16 14	21 49	7 0	−43·7 E.
20	S	16 33	21 6	17 19	−39·9 E.	20	Tu	18 7	22 43	17 53	−57·0 E.	20	Tu	16 45	21 30	16 16	−37·9 E.	20	F	16 36	22 32	2 57 S	−55·9 E.
21	**S**	17 41	22 4	19 5	−52·1 E.	21	W	18 45	23 35	15 31	−69·2 E.	21	W	17 17	22 19	13 21	−50·1 E.	21	S	16 55	23 15	1 11 N	−68·1 E.
22	M	18 41	23 2	19 38	−64·3 E.	22	Th	sets	……	12 23	−81·4 E.	22	Th	17 44	23 6	9 50	−62·3 E.	22	**S**	17 17	23 58	5 14	−80·4 E.
23	Tu	19 31	23 59	18 58	−76·5 E.	23	F	5 42	0 24	8 42	+86·4 M.	23	F	18 8	23 51	5 56	−74·6 E.	23	M	sets	……	9 3	+87·4 M.
24	W	sets	……	17 11	−88·6 E.	24	S	6 51	1 11	4 43	+74·2 M.	24	S	sets	……	1 51 S	−86·8 E.	24	Tu	7 53	0 41	12 30	+75·2 M.
25	Th	5 41	0 53	14 29	+79·2 M.	25	**S**	7 56	1 55	0 36 S	+62·0 M.	25	**S**	6 51	0 34	2 16 N	+81·0 M.	25	W	8 55	1 25	15 26	+62·9 M.
	F	6 52	1 44	11 6	+66·9 M.	26	M	9 3	2 39	3 28 N	+49·8 M.	26	M	7 56	1 17	6 15	+68·8 M.	26	Th	9 56	2 10	17 45	+50·7 M.
		8 1	2 33	7 17	+54·8 M.	27	Tu	10 8	3 22	7 21	+37·6 M.	27	Tu	8 59	2 0	9 57	+56·6 M.	27	F	10 55	2 57	19 18	+38·5 M.

Day.		Moon.				Day.		Moon.				Day.		Moon.				Day.		Moon.			
Month.	*Week.*	Sets.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°			h m	h m	° ′	°			h m	h m	° ′	°			h m	h m	° ′	°
1	Tu	13 54	6 14	16 35 N	−10·4 M.	1	F	13 50	7 28	2 29 N	−28·9 M.	1	**S**	13 12	7 56	10 12 S	−35·5 M.	1	W	13 53	9 36	20 11 S	−54·4 M.
2	W	14 27	7 5	13 35	−22·6 M.	2	S	14 14	8 19	2 26 S	−41·1 M.	2	M	13 43	8 51	14 17	−47·7 M.	2	Th	14 55	10 36	20 4	−66·6 M.
3	Th	14 55	7 55	9 48	−34·8 M.	3	**S**	14 42	9 12	7 21	−53·3 M.	3	Tu	14 22	9 49	17 32	−59·9 M.	3	F	16 5	11 35	18 38	−78·7 M.
4	F	15 21	8 46	5 21	−47·0 M.	4	M	15 13	10 8	11 56	−65·5 M.	4	W	15 11	10 50	19 36	−72·1 M.	4	S	rises	12 32	16 5	+89·1 E.
5	S	15 48	9 39	0 27 N	−59·1 M.	5	Tu	15 49	11 6	15 48	−77·7 M.	5	Th	rises	11 52	20 19	−84·3 M.	5	**S**	8 26	13 25	12 40	+76·9 E.
6	**S**	16 14	10 32	4 37 S	−71·3 M.	6	W	16 34	12 8	18 36	−89·9 M.	6	F	8 33	12 53	19 38	+83·5 E.	6	M	8 53	14 14	8 40	+64·7 E.
7	M	rises	11 28	9 30	−83·5 M.	7	Th	rises	13 10	20 5	+77·9 E.	7	S	9 19	13 51	17 42	+71·3 E.	7	Tu	9 17	15 1	4 22 S	+52·5 E.
8	Tu	7 24	12 26	13 49	+84·3 E.	8	F	9 54	14 11	20 8	+65·7 E.	8	**S**	9 57	14 46	14 45	+59·1 E.	8	W	9 39	15 46	0 0 N	+40·3 E.
9	W	8 47	13 27	17 11	+72·2 E.	9	S	10 45	15 10	18 52	+53·5 E.	9	M	10 27	15 36	11 5	+46·9 E.	9	Th	10 1	16 30	4 16	+28·1 E.
10	Th	10 3	14 28	19 20	+60·0 E.	10	**S**	11 26	16 6	16 29	+41·4 E.	10	Tu	10 52	16 24	7 0	+34·7 E.	10	F	10 22	17 13	8 16	+15·9 E.
11	F	11 11	15 29	20 8	+47·8 E.	11	M	11 59	16 57	13 16	+29·2 E.	11	W	11 16	17 9	2 43 S	+22·5 E.	11	S	10 46	17 57	11 54	+ 3·7 E.
12	S	12 5	16 27	19 36	+35·6 E.	12	Tu	12 25	17 45	9 30	+16·9 E.	12	Th	11 36	17 52	1 33 N	+10·3 E.	12	**S**	11 12	18 42	15 1	− 8·5 E.
13	**S**	12 51	17 23	17 54	+23·4 E.	13	W	12 50	18 30	5 25	+ 4·7 E.	13	F	11 57	18 35	5 41	− 1·9 E.	13	M	11 43	19 28	17 31	−20·8 E.
14	M	13 26	18 14	15 15	+11·2 E.	14	Th	13 9	19 13	1 13 S	− 7·5 E.	14	S	12 19	19 18	9 32	−14·2 E.	14	Tu	12 18	20 16	19 16	−33·0 E.
15	Tu	13 55	19 3	11 56	− 1·0 E.	15	F	13 30	19 56	2 58 N	−19·7 E.	15	**S**	12 44	20 2	12 59	−26·4 E.	15	W	13 0	21 5	20 11	−45·2 E.
16	W	14 20	19 48	8 8	−13·2 E.	16	S	13 51	20 38	6 58	−32·0 E.	16	M	13 10	20 47	15 54	−38·6 E.	16	Th	13 51	21 55	20 10	−57·5 E.
17	Th	14 43	20 32	4 5 S	−25·4 E.	17	**S**	14 14	21 21	10 41	−44·2 E.	17	Tu	13 43	21 34	18 11	−50·9 E.	17	F	14 50	22 46	19 10	−69·7 E.
18	F	15 4	21 15	0 4 N	−37·7 E.	18	M	14 40	22 6	13 59	−56·5 E.	18	W	14 21	22 23	19 41	−63·1 E.	18	S	15 54	23 37	17 10	−82·0 E.
19	S	15 24	21 57	4 11	−49·9 E.	19	Tu	15 10	22 52	16 43	−68·7 E.	19	Th	15 7	23 13	20 18	−75·4 E.	19	**S**	sets	……	14 15	+85·8 M.
20	**S**	15 46	22 40	8 5	−62·1 E.	20	W	15 43	23 39	18 45	−81·0 E.	20	F	16 1	……	19 58	−87·6 E.	20	M	7 38	0 28	10 33	+73·6 M.
21	M	16 9	23 23	11 41	−74·4 E.	21	Th	16 24	……	19 59	+86·8 M.	21	S	sets	0 3	18 38	+80·1 M.	21	Tu	8 5	1 18	6 15	+61·3 M.
22	Tu	16 35	……	14 48	−86·6 E.	22	F	sets	0 28	20 19	+74·5 M.	22	**S**	8 37	0 54	16 22	+67·9 M.	22	W	8 30	2 7	1 33 N	+49·1 M.
23	W	sets	0 8	17 20	+81·1 M.	23	S	9 20	1 18	19 41	+62·3 M	23	M	9 8	1 44	13 15	+55·6 M.	23	Th	8 53	2 57	3 16 S	+36·9 M.
24	Th	8 49	0 54	19 8	+68·9 M.	24	**S**	10 0	2 8	18 6	+50·0 M.	24	Tu	9 35	2 33	9 26	+43·4 M.	24	F	9 19	3 48	7 58	+24·6 M.
25	F	9 45	1 42	20 6	+56·6 M.	25	M	10 34	2 57	15 38	+37·8 M.	25	W	10 0	3 22	5 5	+31·1 M.	25	S	9 47	4 40	12 15	+12·4 M.
26	S	10 34	2 31	20 10	+44·4 M.	26	Tu	11 5	3 47	12 21	+25·6 M.	26	Th	10 25	4 11	0 25 N	+18·9 M.	26	**S**	10 20	5 34	15 50	+ 0·2 M.
27	**S**	11 19	3 20	19 17	+32·2 M.	27	W	11 30	4 35	8 24	+13·3 M.	27	F	10 48	5 0	4 20 S	+ 6·7 M.	27	M	10 59	6 30	18 30	−12·0 M.
28	M	11 56	4 10	17 28	+19·9 M.	28	Th	11 55	5 24	3 58 N	+ 1·1 M.	28	S	11 15	5 50	8 56	− 5·6 M.	28	Tu	11 46	7 28	20 1	−24·2 M.
29	Tu	12 30	5 0	14 46	+ 7·7 M.	29	F	12 19	6 13	0 47 S	−11·1 M.	29	**S**	11 44	6 43	13 6	−17·8 M.	29	W	12 43	8 26	20 16	−36·4 M.
30	W	12 57	5 49	11 17	− 4·5 M.	30	S	12 43	7 3	5 35 S	−23·3 M.	30	M	12 19	7 38	16 32	−30·0 M.	30	Th	13 49	9 24	19 15	−48·6 M.
31	Th	13 25	6 38	7 8 N	−16·7 M.							31	Tu	13 1	8 36	18 59 S	−42·2 M.	31	F	14 59	10 20	17 6 S	−60·8 M.

SEPTEMBER.					
DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	16 12	11 14	13 59 S	−73·0 M.
2	**S**	rises	12 4	10 12	−85·1 M.
3	M	7 18	12 52	5 58	+82·7 E.
4	Tu	7 41	13 38	1 34 S	+70·5 E.
5	W	8 3	14 23	2 49 N	+58·3 E.
6	Th	8 25	15 7	6 59	+46·2 E.
7	F	8 48	15 51	10 48	+34·0 E.
8	S	9 12	16 35	14 7	+21·8 E.
9	**S**	9 41	17 21	16 51	+ 9·6 E.
10	M	10 13	18 8	18 52	− 2·6 E.
11	Tu	10 53	18 56	20 4	−14·8 E.
12	W	11 40	19 46	20 22	−27·1 E.
13	Th	12 35	20 36	19 43	−39·3 E.
14	F	13 36	21 27	18 4	−51·5 E.
15	S	14 45	22 17	15 28	−63·7 E.
16	**S**	15 56	23 8	11 59	−76·0 E.
17	M	17 11	23 59	7 48	−88·2 E.
18	Tu	sets	……	3 5 N	+79·6 M.
19	W	6 56	0 50	1 52 S	+67·4 M.
20	Th	7 21	1 41	6 47	+55·1 M.
21	F	7 49	2 34	11 20	+42·9 M.
22	S	8 21	3 29	15 12	+30·7 M.
23	**S**	8 58	4 25	18 9	+18·5 M.
24	M	9 43	5 23	19 56	+ 6·3 M.
25	Tu	10 35	6 21	20 28	− 5·9 M.
26	W	11 39	7 18	19 45	−18·1 M.
27	Th	12 47	8 14	17 52	−30·3 M.

OCTOBER.					
DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	17 30	11 32	3 0 S	−79·0 M.
2	Tu	rises	12 17	1 26 N	+88·9 E.
3	W	6 28	13 1	5 43	+76·7 E.
4	Th	6 49	13 45	9 42	+64·6 E.
5	F	7 13	14 29	13 15	+52·4 E.
6	S	7 39	15 15	16 13	+40·2 E.
7	**S**	8 10	16 1	18 30	+28·1 E.
8	M	8 48	16 49	19 59	+15·9 E.
9	Tu	9 30	17 37	20 35	+ 3·7 E.
10	W	10 21	18 26	20 16	− 8·5 E.
11	Th	11 20	19 16	18 59	−20·7 E.
12	F	12 23	20 6	16 45	−32·9 E.
13	S	13 34	20 56	13 37	−45·1 E.
14	**S**	14 47	21 46	9 42	−57·3 E.
15	M	16 3	22 37	5 8	−69·5 E.
16	Tu	17 24	23 29	0 9 N	−81·7 E.
17	W	sets	……	4 57 S	+86·1 M.
18	Th	5 47	0 22	9 50	+73·8 M.
19	F	6 18	1 18	14 10	+61·6 M.
20	S	6 54	2 15	17 34	+49·4 M.
21	**S**	7 38	3 15	19 48	+37·2 M.
22	M	8 29	4 15	20 42	+25·0 M.
23	Tu	9 29	5 14	20 16	+12·8 M.
24	W	10 38	6 11	18 37	+ 0·7 M.
25	Th	11 49	7 5	15 57	−11·5 M.
26	F	13 0	7 56	12 31	−23·7 M.
27	S	14 11	8 44	8 32	−35·9 M.

NOVEMBER.					
DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	5 17	12 25	12 22 N	+83·4 E.
2	F	5 41	13 10	15 34	+71·3 E.
3	S	6 10	13 56	18 6	+59·1 E.
4	**S**	6 44	14 43	19 51	+47·0 E.
5	M	7 24	15 31	20 45	+34·8 E.
6	Tu	8 12	16 20	20 44	+22·6 E.
7	W	9 7	17 8	19 46	+10·5 E.
8	Th	10 6	17 57	17 53	− 1·7 E.
9	F	11 14	18 46	15 7	−13·9 E.
10	S	12 22	19 34	11 33	−26·0 E
11	**S**	13 36	20 23	7 17	−38·2 E.
12	M	14 52	21 14	2 31 N	−50·4 E.
13	Tu	16 12	22 6	2 34 S	−62·6 E.
14	W	17 34	23 0	7 39	−74·8 E.
15	Th	19 0	23 58	12 23	−87·0 E.
16	F	sets	……	16 22	+80·8 M.
17	S	5 26	0 58	19 14	+68·6 M.
18	**S**	6 16	2 0	20 44	+56·4 M.
19	M	7 16	3 2	20 47	+44·2 M.
20	Tu	8 22	4 2	19 27	+32·0 M.
21	W	9 35	4 59	16 59	+19·8 M.
22	Th	10 49	5 53	13 39	+ 7·6 M.
23	F	12 1	6 42	9 43	− 4·5 M.
24	S	13 10	7 29	5 27	−16·7 M.
25	**S**	14 19	8 13	1 2 S	−28·8 M.
26	M	15 27	8 57	3 21 N	−41·0 M.
27	Tu	16 33	9 40	7 33	−53·1 M.

DECEMBER.					
DAY.		MOON.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	4 44	12 40	19 34 N	+70·4 E.
2	**S**	5 22	13 28	20 45	+66·2 E.
3	M	6 7	14 16	21 0	+54·1 E.
4	Tu	6 59	15 5	20 19	+41·9 E.
5	W	7 58	15 53	18 43	+29·8 E.
6	Th	9 0	16 41	16 14	+17·7 E.
7	F	10 7	17 28	12 58	+ 5·5 E.
8	S	11 18	18 15	9 1	− 6·7 E.
9	**S**	12 29	19 3	4 32 N	−18·8 E.
10	M	13 44	19 52	0 19 S	−31·0 E.
11	Tu	15 3	20 43	5 18	−43·2 E.
12	W	16 25	21 38	10 10	−55·4 E.
13	Th	17 48	22 36	14 32	−67·6 E.
14	F	19 11	23 37	18 2	−79·7 E.
15	S	sets	……	20 17	+88·1 M.
16	**S**	4 54	0 41	21 3	+75·9 M.
17	M	6 0	1 44	20 19	+63·7 M.
18	Tu	7 14	2 46	18 13	+51·5 M.
19	W	8 30	3 43	15 3	+39·3 M.
20	Th	9 45	4 36	11 10	+27·2 M.
21	F	10 57	5 25	6 52	+15·0 M.
22	S	12 7	6 11	2 23 S	+ 2·8 M.
23	**S**	13 17	6 55	2 5 N	− 9·3 M.
24	M	14 23	7 39	6 23	−21·5 M.
25	Tu	15 29	8 22	10 22	−33·6 M.
26	W	16 34	9 5	13 54	−45·8 M.
27	Th	17 36	9 50	16 51	−57·9 M.

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. Denning.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	a °	δ °			*a* °	δ °	
an. 2-3. .	**230**	**+53**	Swift; long paths.	July-Sept. .	335	+73	Swift; short.
3 ...	156	+41	Swift.	July-Aug. .	339	−27	Slow; long.
11 ...	220	+13	Swift; streaks.	July-Oct. 8	30	+36	Swift; streaks.
17 ...	295	+53	Slow; bright.	Aug. 4-10	350	+49	Swift.
17-25	143	+48	Swift; white.	10-12	**45**	**+57**	Swift; streaks.
25 ...	131	+32	Swift.	10-Sept. 16	353	−11	Rather slow.
29 ...	213	+52	Very swift.	Aug. 15 ...	290	+53	Swift; bright.
eb. 5-10	75	+41	Slow; bright.	21-25	291	+60	Slow; bright.
15 ...	236	+11	Swift; streaks.	25 ...	5	+11	Slow; short.
15 ...	261	+ 4	Swift; streaks.	Aug.-Sept. .	346	± 0	Slow.
20 ...	181	+34	Swift; bright.	Aug.-Oct. 2	74	+42	Swift; streaks.
20 ...	263	+36	Swift; streaks.	Sept. 3-8 .	353	+39	Very swift.
lar. 1-4...	166	+ 4	Slow; bright.	5-15	62	+37	Swift; streaks.
14 ...	250	+54	Swift.	6-17	106	+52	Swift; streaks.
18 ..	316	+76	Slow; bright.	15 ...	77	+57	Swift; streaks.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow; trains.
27 ...	229	+32	Swift; small.	27 ...	75	+15	Swift; streaks.
r.-Ap.-May	263	+62	Rather swift.	Sep. 29-Oct. 5	189	+73	Swift.
pr. 12-24	210	−10	Slow; fireballs.	Oct. 2 ...	230	+52	Slow; bright.
.17-May 1	240	+47	Small; short.	4 ...	310	+79	Slowish.
pr. 18-23	189	−31	Slow; long.	8 ...	77	+31	Swift; streaks.
20-21	261	+36	Swift; bl. white.	8-14	45	+58	Small; short.
20-22	**271**	**+33**	Swift.	14 ...	133	+68	Rather swift.
20-25	218	−31	Slow; long paths.	16-23	89	+ 8	Swift; streaks.
30 ...	291	+59	Rather slow.	18-20	**92**	**+15**	Swift; streaks.
pr.-May .	193	+58	Slow; yellow.	23 ...	99	+13	Swift; streaks.
lay 1-6.	**338**	**− 2**	Swift; streaks.	29 ...	109	+23	Very swift.
11-18	231	+27	Slow; small.	Nov. 1 ...	43	+22	Slow; bright.
25-June 4	280	+31	Swift; white.	2 ...	55	+ 9	Slow; bright.
30-Aug.	333	+27	Swift; streaks.	10-12	133	+31	Very swift; streaks.
lay-June .	235	+ 9	Rather slow.	14-16	**150**	**+22**	Swift; streaks.
lay-July .	252	−21	Slow; trains.	16-28	154	+41	Swift; streaks.
une 10 ...	261	+ 5	Very slow.	20-23	63	+23	Slow; bright.
11-19	274	+69	Rather swift.	17-23	**25**	**+43**	Very slow; trains.
13 ...	310	+61	Swift; streaks.	25-Dec. 12	316	+60	Slowish.
une-Sep...	335	+57	Swift; slow in Sep.	Nov. 30 ...	190	+58	Swift; streaks.
ne 22-July 1	245	+64	Slow.	Dec. 4 ...	162	+58	Swift; streaks.
une-Aug. .	303	+24	Swift.	6 ...	80	+23	Slow; bright.
uly 13-20	317	+31	Swift.	8 ...	145	+ 7	Swift; streaks.
15-31	23	+43	Swift; streaks.	8 ...	208	+71	Rather swift.
19 ...	315	+48	Swift; short.	10-12	**108**	**+33**	Swift; short.
uly-Aug. .	269	+48	Slow; trains.	12 ...	119	+29	Rather swift.
15-30	305	−10	Slow; bright.	22 ...	194	+67	Swift; streaks.
25-Sep. 15	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.
uly 28 ...	**339**	**−11**	Slow; long.	31 ...	92	+57	Slow; bright.

The radiant-points of the more brilliant showers are indicated by heavier type. The ndromedids of November may be very plentiful in 1906, November 18–21.

The Perseids, with max. on August 11, are visible for a considerable period and their radiant-int exhibits a motion to E.N.E. amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	a °	δ °		*a* °	δ °		*a* °	δ °
July 19 ...	18·9	+50·5	July 29 ...	29·3	+53·8	Aug. 8 ...	41·5	+56·5
21 ...	20·8	+51·1	31 ...	31·6	+54·4	10 ...	44·3	+56·9
23 ...	22·8	+51·8	Aug. 2 ...	33·9	+55·0	12 ...	47·1	+57·3
25 ...	24·9	+52·5	4 ...	36·4	+55·5	14 ...	50·0	+57·7
27 ...	27·1	+53·2	6 ...	38·9	+56·0	16 ...	52·9	+58·0

1906.	Mercury (☿).						Venus (♀).						Mars (♂).					
	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1...	17 7 27	20 35 S.	7·2	18 15	22 26	2 37	17 58 32	23 26 S.	10·0	19 25	23 19	3 12	22 24 8	11 2 S.	5·4	22 36	3 43	8 50
11...	17 52 26	22 36	6·0	18 30	22 33	2 32	18 53 22	23 16	10·0	19 39	23 34	3 29	22 52 41	8 3	5·2	22 4	3 32	9 0
21...	18 51 41	23 25	5·2	19 1	22 54	2 47	19 47 32	21 55	9·8	19 46	23 49	3 52	23 20 44	4 58	5·0	21 43	3 21	8 59
31...	19 56 50	22 17	5·0	19 20	23 20	3 20	20 40 15	19 28	9·8	19 42	0 1	4 20	23 48 24	1 50 S.	4·8	21 14	3 9	9 4
Feb. 10...	21 4 35	18 52	4·8	19 24	23 49	4 14	21 30 59	16 4	9·8	19 32	0 12	4 52	0 15 49	1 18 N.	4·8	20 47	2 57	9 7
20...	22 13 39	13 4	4·8	19 20	0 10	5 12	22 19 44	11 56	9·8	19 19	0 22	5 27	0 43 6	4 22	4·6	20 19	2 45	9 11
Mar. 2...	23 22 58	5 3 S.	5·2	19 8	0 46	6 24	23 6 48	7 15	9·8	19 2	0 29	5 56	1 10 23	7 21	4·4	19 52	2 33	9 14
12...	0 26 10	3 50 N.	6·2	18 46	1 9	7 32	23 52 41	2 16 S.	9·8	18 43	0 36	6 29	1 37 47	10 11	4·4	19 25	2 21	9 17
22...	1 3 18	9 53	8·4	18 13	1 7	8 1	0 38 6	2 50 N.	10·0	18 24	0 42	7 0	2 5 22	12 51	4·2	18 59	2 9	9 19
Apr. 1...	0 58 23	9 42	10·8	17 31	0 23	7 15	1 23 43	7 51	10·0	18 4	0 48	7 32	2 33 15	15 18	4·2	18 31	1 57	9 22
11...	0 34 22	4 53	11·4	16 47	23 14	5 41	2 10 11	12 33	10·2	17 46	0 55	8 4	3 1 28	17 31	4·0	18 9	1 46	9 23
21..	0 31 6	1 48	10·0	16 22	22 34	4 46	2 58 6	16 46	10·4	17 31	1 4	8 37	3 30 1	19 27	4·0	17 45	1 35	9 25
May 1...	0 54 33	2 47	8·2	16 4	22 20	4 36	3 47 47	20 16	10·6	17 17	1 14	9 9	3 58 54	21 5	3·8	17 25	1 25	9 25
11...	1 36 38	6 50	7·0	15 45	22 24	5 3	4 39 12	22 52	10·8	17 13	1 26	9 39	4 28 3	22 24	3·8	17 3	1 14	9 25
21...	2 33 50	12 47	5·8	15 32	22 44	5 56	5 31 57	24 24	11·2	17 15	1 39	10 3	4 57 23	23 22	3·8	16 48	1 4	9 19
31...	3 47 59	19 20	5·2	15 29	23 20	7 11	6 25 16	24 45	11·6	17 26	1 53	10 20	5 26 48	24 0	3·6	16 33	0 54	9 15
June 10...	5 19 2	24 10	5·0	15 43	0 7	8 31	7 18 4	23 56	12·0	17 46	2 7	10 28	5 56 8	24 17	3·6	16 21	0 44	9 7
20...	6 51 23	24 52	5·4	16 32	1 0	9 28	8 9 25	21 58	12·4	18 12	2 18	10 24	6 25 18	24 13	3·6	16 12	0 34	8 56
30...	8 7 36	21 53	6·2	17 30	1 37	9 44	8 58 41	19 2	13·0	18 41	2 28	10 15	6 54 7	23 49	3·6	16 4	0 23	8 42
July 10...	9 3 33	17 9	7·2	18 19	1 54	9 29	9 45 32	15 16	13·8	19 12	2 36	10 0	7 22 29	23 6	3·6	15 59	0 12	8 25
20...	9 38 28	12 23	8·6	18 41	1 49	8 57	10 30 6	10 53	14·6	19 41	2 41	9 41	7 50 19	22 5	3·6	15 55	0 1	8 [illegible]
30...	9 47 55	9 17	10·2	18 28	1 19	8 10	11 12 42	6 5	15·6	20 9	2 44	9 19	8 17 33	20 48	3·6	15 49	23 47	7 45
Aug. 9...	9 28 18	9 48	11·0	17 28	0 20	7 12	11 53 45	1 3 N.	16·8	20 37	2 46	8 55	8 44 9	19 16	3·6	15 47	23 35	7 23
19...	9 3 20	13 22	9·8	15 59	23 11	6 23	12 33 46	4 3 S.	18·2	21 3	2 46	8 29	9 10 6	17 31	3·6	15 44	23 21	6 58
29...	9 17 28	15 21	7·4	15 20	22 51	6 16	13 13 8	9 1	19·8	21 28	2 46	8 4	9 35 27	15 35	3·6	15 41	23 7	6 33
Sept. 8...	10 14 25	12 30	5·8	16 2	23 11	6 20	13 52 6	13 42	21·8	21 51	2 45	7 40	10 0 13	13 29	3·6	15 38	22 52	6 6
18..	11 24 30	5 45 N.	5·0	17 7	23 41	6 15	14 30 37	17 57	24·2	22 17	2 45	7 13	10 24 30	11 15	3·6	15 36	22 37	5 38
28...	12 30 9	2 5 S.	4·8	18 11	0 5	5 59	15 8 15	21 35	27·4	22 38	2 43	6 48	10 48 20	8 55	3·8	15 32	22 21	5 10
Oct. 8...	13 30 40	9 30	4·8	19 10	0 26	5 42	15 43 46	24 29	31·2	22 54	2 39	6 24	11 11 49	6 30	3·8	15 29	22 5	4 41
18...	14 28 38	15 54	5·0	20 3	0 44	5 25	16 15 7	26 32	36·2	22 59	2 31	6 3	11 35 2	4 2	3·8	15 26	21 49	4 12
28...	15 25 20	20 56	5·4	20 52	1 2	5 12	16 38 58	27 40	42·6	22 54	2 15	5 36	11 58 4	1 32 N.	4·0	15 22	21 33	3 44
Nov. 7...	16 18 10	24 9	6·2	21 27	1 15	5 3	16 50 41	27 45	50·2	22 27	1 47	5 7	12 21 0	0 57 S.	4·0	15 17	21 16	3 15
17...	16 53 41	24 52	7·8	21 30	1 11	4 52	16 46 11	26 32	58·0	21 32	1 3	4 34	12 43 56	3 25	4·2	15 14	21 0	2 46
27...	16 36 49	21 55	9·8	20 14	0 15	4 16	16 26 40	23 50	63·0	20 14	0 5	3 55	13 6 54	5 50	4·4	15 11	20 44	2 17
Dec. 7...	15 52 28	17 30	8·8	18 16	22 46	3 16	16 3 54	20 18	61·6	18 45	22 57	3 9	13 29 59	8 11	4·4	15 5	20 27	1 49
17...	16 6 42	18 32	6·8	18 1	22 26	2 51	15 52 11	17 31	55·0	17 37	22 7	2 37	13 53 14	10 25	4·6	15 1	20 11	1 21
27...	16 55 25	21 34 S.	5·6	18 32	22 37	2 42	15 56 11	16 22 S.	47·0	16 56	21 33	2 10	14 16 41	12 32 S.	4·8	14 57	19 55	0 53

Mercury.

	d h	d h	d h
Gr. elong. E.	Mar. 18 5;	July 15 3;	Nov. 9 9
" W.	Jan. 4 15;	Aug. 29 11;	Dec. 18 5
Sup. conj. ...	May 2 17;	Feb. 20 9;	June 8 9

Venus.

	d h	d h
Gr. elong. E.	Sept. 20 10	
Sup. conj. ...	Feb. 13 21	
Inf. conj. ...	Nov. 29 17	

Superior Planets.

	Conjunction.	Opposition.	Quadrature.		Stationary.	
	d h	d h	d h	d h	d h	d h
Mars......	July 15 8					
Jupiter...	June 10 3	Dec. 28 3	Feb. 16 17;	Oct. 3 15	Jan. 21 10;	Oct. 29 13

Jupiter (♃). Saturn (♄).

1906.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Se
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h
an. 1	3 40 17	18 41 N.	42·6	1 15	8 58	16 41	22 7 58	13 9 S.	14·8	22 32	3 26	8
21	3 37 23	18 36	40·2	23 54	7 36	15 18	22 15 48	12 25	14·6	21 17	2 16	7
eb. 10	3 40 3	18 50	37·6	22 36	6 20	14 5	22 24 37	11 35	14·4	20 3	1 6	6
Mar. 2	3 47 49	19 20	35·2	21 22	5 10	12 58	22 33 50	10 42	14·4	18 45	23 53	5
22	3 59 47	20 0	33·2	20 11	4 3	11 55	22 42 53	9 50	14·4	17 30	22 43	3
pr. 11	4 14 59	20 45	31·8	19 2	2 59	10 56	22 51 12	9 3	14·6	16 16	21 33	2
May 1	4 32 30	21 28	30·8	17 56	1 58	10 0	22 58 16	8 23	15·0	15 1	20 21	1
21	4 51 31	22 6	30·0	16 52	0 58	9 4	23 3 39	7 54	15·4	13 45	19 8	0
une 10	5 11 19	22 35	29·8	15 48	23 57	8 6	23 6 55	7 39	16·0	12 28	17 52	23
30	5 31 12	22 56	30·0	14 46	22 58	7 10	23 7 48	7 39	16·6	11 10	16 34	21
uly 20	5 50 26	23 6	30·6	13 45	21 58	6 11	23 6 15	7 54	17·0	9 51	15 14	20
ug. 9	6 8 17	23 8	31·4	12 44	20 57	5 10	23 2 32	8 21	17·4	8 32	13 52	19
29	6 23 56	23 3	32·8	11 42	19 54	4 6	22 57 21	8 56	17·6	7 11	12 28	17
ept. 18	6 36 27	22 55	34·6	10 37	18 48	2 59	22 51 43	9 31	17·6	5 49	11 4	16
ct. 8	6 44 48	22 48	36·6	9 27	17 37	1 47	22 46 49	10 0	17·4	4 28	9 40	14
28	6 48 3	22 46	39·0	8 12	16 22	0 32	22 43 39	10 17	17·0	3 9	8 19	13
ov. 17	6 45 38	22 51	41·2	6 50	15 0	23 10	22 42 54	10 18	16·4	1 49	6 59	12
ec. 7	6 37 55	23 1	43·0	5 22	13 34	21 46	22 44 46	10 4	15·8	0 30	5 42	10
27	6 26 49	23 12 N.	43·6	3 52	12 4	20 16	22 49 6	9 35 S.	15·4	23 14	4 28	9

Uranus (♅). Neptune (♆).

an. 1	18 20 36	23 38 S.	3·3	19 45	23 36	3 27	6 38 49	22 11 N.	2·7	3 50	11 56	20
31	18 28 4	23 34	3·4	17 53	21 45	1 37	6 35 24	22 14	2·6	1 49	9 55	18
Mar. 2	18 33 49	23 30	3·5	16 1	19 53	23 45	6 33 15	22 17	2·6	23 47	7 54	16
pr. 1	18 36 47	23 28	3·5	14 6	17 58	21 50	6 33 6	22 19	2·6	21 48	5 56	14
May 1	18 36 28	23 29	3·6	12 8	16 0	19 52	6 35 4	22 18	2·6	19 54	4 0	12
31	18 33 10	23 33	3·8	10 7	13 59	17 51	6 38 44	22 16	2·5	17 59	2 6	10
une 30	18 28 10	23 37	3·8	8 3	11 55	15 47	6 43 22	22 12	2·5	16 7	0 13	8
uly 30	18 23 13	23 40	3·6	6 2	9 53	13 44	6 48 3	22 7	2·5	14 11	22 16	6
ug. 29	18 20 7	23 41	3·6	4 1	7 52	11 43	6 51 56	22 2	2·6	12 16	20 21	4
ept. 28	18 20 3	23 41	3·6	2 2	5 53	9 43	6 54 13	21 58	2·6	10 21	18 26	2
ct. 28	18 23 14	23 39	3·4	0 8	3 59	7 50	6 54 28	21 57	2·6	8 24	16 28	0
ov. 27	18 29 7	23 35	3·4	22 16	2 7	5 58	6 52 37	21 59	2·7	6 23	14 28	22
ec. 27	18 36 36	23 30 S.	3·3	20 24	0 16	4 8	6 49 22	22 3 N.	2·7	4 22	12 27	20

Ceres. Pallas.

1906.	R.A. Noon.	Dec. Noon.	Transits.	1906.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m		h m	° ′	h m
Nov. 18 ...	5 6	19 39 S.	13 17	Sept. 15 ...	1 11	6 56 S.	13 34
26 ...	4 59	19 56	12 39	23 ...	1 6	9 7	12 58
Dec. 4 ...	4 51	20 13	11 59	Oct. 1 ...	1 0	11 18	12 20
12 ...	4 43	20 30	11 20	9 ...	0 54	13 22	11 43
20 ...	4 35	20 47 S.	10 40	17 ...	0 48	15 14 S.	11 5

Juno. Vesta.

Jan. 18 ...	9 15	1 19 N.	13 24	Aug. 22 ...	23 53	12 4 S.	13 51
26 ...	9 8	2 17	12 46	30 ...	23 47	13 9	13 14
Feb. 3 ...	9 2	3 28	12 8	Sept. 7 ...	23 40	14 13	12 35
11 ...	8 55	4 47	11 30	15 ...	23 33	15 10	11 57
19 ...	8 49	6 10 N.	10 53	23 ...	23 26	15 57 S.	11 18

The times of rising and setting correspond to the adjacent transit, irrespective of the day given i
e first column.

ECLIPSES IN 1906.

In the year 1906 there will be three Eclipses of the Sun and two of the Moon.

I.—*A Total Eclipse of the Moon*, 1906, *February* 8.

Partly visible at Greenwich. The whole of the Total Phase will be seen from any point in North and South America. The Moon will set at Greenwich, totally eclipsed, at 19^h 30^m.

	h m		h m
First contact with Penumbra	16 54·2	With shadow	17 57·1
Last ,, ,, ...	22 40·0	,, ,,	21 37·1
Beginning of Total Phase	18 57·9		
End ,, ,, ...	20 36·3		

First contact at 96°; Last contact at 289°, from North point to East.
Magnitude of the Eclipse 1·632 (Moon's diam. = 1).

II.—*A Partial Eclipse of the Sun*, 1906, *February* 22.

Invisible at Greenwich. Visible from the Antarctic and S. Indian Oceans, and in the southern part of Australia, including Adelaide and Melbourne. Begins 17^h 57^m·8, in longitude 20° 32′ W., latitude 66° 51′ S., and ends 21^h 20^m·0, in longitude 138° 51′ E., latitude 37° 2′ S. Greatest Eclipse 0·540 (Sun's diam. = 1) at 19^h 43^m·4, in longitude 170° 22′ W., latitude 71° 31′ S.

III.—*A Partial Eclipse of the Sun*, 1906, *July* 20-21.

Invisible at Greenwich. This also is visible only from the S. Atlantic and Antarctic Oceans, the Falkland Islands, and S. Patagonia. Begins 20^d 23^h 48^m·5, in longitude 58° 36′ W., latitude 50° 33′ S. Ends 21^d 2^h 40^m·1, in longitude 11° 37′ E., latitude 59° 48′ S. Greatest Eclipse 0·337 (Sun's diam. = 1) at 21^d 1^h 14^m·3, in longitude 33° 27′ W., latitude 68° 44′ S.

IV.—*A Total Eclipse of the Moon*, 1906, *August* 3-4.

Invisible at Greenwich. The Total Phase will be seen from India (in part), Further India, China, Australia, and the Pacific Ocean.

	d h m		d h m
First contact with Penumbra	3 22 11·8	With shadow	3 23 10·6
Last ,, ,, ...	4 3 48·6	,, ,,	4 2 49·8
Beginning of Total Phase......	4 0 9·3	End............	4 1 51·1

Magnitude of the Eclipse 1·787 (Moon's diam. = 1).
First contact at 82° from the North point; Last at 257° towards the East in both cases.

V.—*A Partial Eclipse of the Sun*, 1906, *August* 19.

Invisible at Greenwich. This Eclipse is visible only from the region round the North Pole, as far south as the extreme North of Siberia and including the North-west of N. America as far south as Vancouver Island. The line of simple contact passes through Behring Straits. Begins 11^h 53^m·1, in longitude 49° 15′ E., latitude 71° 46′ N. Ends 14^h 32^m·4, in longitude 113° 16′ W., latitude 46° 49′ N. Greatest Eclipse 0·315 (Sun's diam. = 1) at 13^h 12^m·7, in longitude 66° 12′ W., latitude 71° N.

OCCULTATIONS, 1906. (VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap. M. T.	Disap. P.	Reap. M. T.	Reap. P.
Jan.			h m	°	h m	°
4	ξ² Ceti............	4·3	4 12	104	5 10	207
5	B.D.+12° 473.	6·2	10 48	116	11 44	213
5	*f* Tauri	4·3	14 45	137	15 16	203
6	B.D.+14° 657.	5·5	7 14	75	8 34	248
6	48 Tauri.........	6·4	12 57	154	13 17	188
6	γ Tauri	3·9	14 52	117	15 40	230
7	B.A.C. 1526 ...	5·8	6 53	100	8 2	230
10	*g* Geminorum...	5·1	12 5	28	12 32	348
11	*o*′ Cancri.........	5·2	20 22	120	21 11†	267
14	σ Leonis.........	4·1	11 1	134	11 58	264
20	29 Ophiuchi ...	6·5	16 13†	155	16 52	235
Feb.						
3	α Tauri	1·1	5 23	40	6 28	287
4	115 Tauri	5·4	5 25	103	6 33	232
4	120 Tauri	5·3	9 54	18	10 26	331
7	ζ Cancri.........	4·7	7 7	60	8 4	308
7	*d*² Cancri	6·2	14 53	62	15 39	327
8	π² Cancri	5·6	11 12	81	12 21	311
9	ν Leonis.........	5·2	5 15†	57	5 52	324
10	χ Leonis.........	4·7	11 7	134	12 12	269
11	10 Virginis......	6·1	17 12	63	17 52	346
13	*n* Virginis	7·0	11 36	154	12 21	252
13	B.A.C. 4665 ...	6·5	18 22	155	19 15	251
14	ξ′ Libræ.........	5·9	17 17	69	18 11	334
16	B.A.C. 5579 ...	5·1	13 28†	80	14 21*	311
18	29 Sagittarii ...	5·5	18 40	50	19 46	272
19	*f* Sagittarii......	5·1	17 53	120	18 52	238
26	33 Ceti	6·3	5 44	29	6 40	285
28	μ Ceti	4·4	7 0	54	8 9	271
Mar.						
1	*f* Tauri	4·3	6 14	121	7 9	205
2	γ Tauri	3·9	6 41	116	7 45	218
2	θ¹ Tauri.........	3·9	11 51	114	12 40	235
2	B.D.+15° 633.	6·5	12 2	69	12 57	280
2	θ² Tauri.........	3·6	12 3	144	12 31	204
2	B.A.C. 1391 ...	4·9	12 46	69	13 38†	280
4	B.D.+18° 1129	6·5	13 41	112	14 33	254
6	*g* Geminorum...	5·1	5 47	15	6 3	349
7	*o*² Cancri	5·6	14 35	84	15 30	307
7	*o*¹ Cancri	5·2	14 40	147	15 23	244
11	γ Virginis (mean)	3·0	16 11	156	16 59	252
14	γ Libræ	4·1	17 22	75	18 23	318
17	21 Sagittarii ...	4·9	13 41†	142	14 25*	234
29	B.D.+14° 657.	6·5	9 1	115	9 53	230
29	α Tauri	1·1	20 56	33	21 35	300
pr.						
3	*d*² Cancri	6·2	9 38	122	10 47	267
4	π² Cancri	5·6	6 37	119	7 52	271
5	α Leonis.........	1·3	5 48	67	6 24	327
5	45 Leonis	5·9	16 35†	170	16 58†	225
6	χ Leonis.........	4·7	7 3	149	7 57	253
6	σ Leonis.........	4·1	15 50	53	16 19	348
7	10 Virginis......	6·1	12 6	75	12 58	337

Date.	Star.	Mag.	Disap. M. T.	Disap. P.	M.
Apr.			h m	°	h
9	B.A.C. 4666 ...	6·5	10 27	172	11
10	ξ¹ Libræ.........	5·9	7 54†	90	8
11	49 Libræ	5·6	10 56	165	11
12	29 Ophiuchi ...	6·5	10 44†	93	11
13	15 Sagittarii ...	5·6	16 48	104	18
16	θ Capricorni ...	4·2	15 48	135	16
18	70 Aquarii......	6·3	16 0	114	16
27	119 Tauri	4·6	8 51	83	9
27	120 Tauri	5·3	9 28	95	10
30	ζ Cancri.........	4·7	11 33	110	12
May					
2	23 Leonis	6·5	7 52	107	9
2	ν Leonis.........	5·2	11 59	94	12
5	γ Virginis (mean)	3·0	13 33	175	14
8	η Libræ	5·5	16 17	94	17
June					
5	49 Libræ.........	5·6	7 57	125	9
7	μ Sagittarii......	4·0	10 45	116	11
10	ι Capricorni ...	4·3	13 43	109	14
11	45 Aquarii	6·3	15 28	129	16
30	*n* Virginis	7·0	11 26	112	12
July					
2	γ Libræ	4·1	7 55	73	8
2	η Libræ	5·5	12 34	121	13
5	33 Sagittarii ...	6·0	13 40	143	14
5	ξ² Sagittarii ...	3·5	15 15	98	16
7	19 Capricorni...	6·0	12 38	8	13
7	21 Capricorni...	6·3	16 0	11	16
15	*f* Tauri	4·3	13 34	61	14
16	48 Tauri.........	6·4	12 21†	95	13
16	γ Tauri	3·9	14 10	100	15
Aug.					
1	15 Sagittarii ...	5·6	6 39	52	7
4	ι Capricorni ...	4·3	9 3	78	10
5	45 Aquarii......	6·3	9 40	76	10
6	ψ¹ Aquarii	4·5	12 21	130	12
10	ξ² Ceti	4·3	13 18	27	14
12	B.D.+14° 657.	6·5	16 12	97	17
26	49 Libræ.........	5·6	7 50	111	9
28	B.A.C. 6098 ...	6·5	9 34	71	10
29	ξ² Sagittarii ...	3·5	6 29	88	7
31	19 Capricorni...	6·0	6 10	17	6
Sept.					
1	ι Aquarii	4·3	14 42	70	15
9	75 Tauri.........	5·3	10 19	43	11
9	B.D.+15° 633.	6·5	10 26	98	11
9	B.A.C. 1391 ...	4·9	11 14	116	11
9	α Tauri	1·1	14 27	149	14
22	η Libræ	5·5	7 1	101	8

† Star below horizon. * Star rising. ‡ Star setting.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from t *bottom of the Moon's* inverted image towards the right.

Star.	Mag.	Disap.		Reap.	
		M.T.	P.	M.T.	P.
		h m	°	h m	°
B.A.C. 81	6·0	16 28	49	17 25‡	266
26 Ceti	6·0	11 28	356	12 0	308
μ Ceti	4·4	15 35	75	16 52	243
B.D. +12° 473 .	6·2	9 55	96	10 55	220
B.D. +14° 657 .	6·5	7 48	116	8 28	211
χ^3 Orionis	5·1	17 18	97	18 40	257
θ Cancri	5·6	13 28	37	14 2	330
B.A.C. 6671 ...	6·5	8 35	158	8 49	183
ι Capricorni ...	4·3	6 49	348	6 56	338
39 Aquarii	6·5	4 7	123	4 54	206
45 Aquarii	6·3	8 16	48	9 29	266
ν Geminorum...	4·1	11 24	67	12 34	276
d^1 Cancri	6·0	18 58	133	20 5	256
80 Virginis	5·8	18 30	125	19 34	287
ο Sagittarii......	3·9	5 30	100	6 34	247

Date.	Star.	Mag.	Disap.		R
			M.T.	P.	M.T.
Dec.			h m	°	h m
2	χ^3 Orionis	5·1	5 33	111	6 19
2	ν Geminorum .	4·1	19 47	9c	20 43
3	ζ Geminorum .	var.	10 36	23	11 1c
5	B.A.C. 3029 ...	6·5	16 20	160	17 8
19	γ Capricorni ...	3·8	4 40	104	5 39
19	δ Capricorni ...	3·0	8 32‡	137	8 54
21	B.A.C. 8214 ...	6·5	9 57	85	10 55
25	μ Ceti	4·4	10 59	55	12 12
26	B.D. +12° 473.	6·2	5 28	106	6 23
27	63 Tauri	5·6	13 15	55	14 21
28	*m* Tauri	5·1	9 35	67	10 57
29	B.D. +19° 1110	6·1	6 29	51	7 28
29	χ^2 Orionis	5·9	7 47	108	8 51
29	χ^4 Orionis	4·8	13 32	84	14 48

† Star below horizon. ‡ Star setting.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i.e.* from the bottom of the Moon's inverted image towards the right.

following "near approaches" are also given in the 'Nautical Almanac,' and are espe interesting to observers rather South and rather North of Greenwich respectively:-

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle
			h m	°				h m	°
Mar. 2	75 Tauri	5·3	12 16	354	Aug. 31	21 Capricorni...	6·3	9 48	347
19	π Capricorni ...	5·2	17 54	354	31	θ Capricorni ...	4·2	12 44	342
Apr. 13	16 Sagittarii ...	6·2	17 24	2	Oct. 8	χ^2 Orionis	5·9	12 43	348
May 8	γ Libræ	4·1	12 19	20	Nov. 28	μ Ceti	4·4	3 48	340
June 6	29 Ophiuchi ...	6·5	7 37	15	30	63 Tauri	5·6	5 40	343
7	15 Sagittarii ...	5·6	11 56	6	Dec. 2	χ^4 Orionis	4·8	6 15	351
9	σ Capricorni ...	5·6	11 42	356	5	δ Cancri	4·2	10 6	4
					30	ζ Geminorum...	var.	18 48	7

South Near Approaches.

			h m	°				h m	°
Jan. 10	3 Cancri	6·0	19 57	190	July 17	α Tauri	1·1	0 40	172
Mar. 6	3 Cancri	6·0	14 22	192	Sept. 9	θ′ Tauri	3·9	10 35	165
12	65 Virginis	6·1	8 46	201	11	71 Orionis	5·1	13 52	171
30	B.A.C. 1526 ...	5·8	9 55	176	Oct. 27	ψ′ Aquarii	4·5	12 21	155
Apr. 26	α Tauri	1·1	5 42	172	Nov. 24	B.A.C. 8274 ...	6·4	9 40	153
May 31	σ Leonis	4·1	9 54	205	Dec. 2	68 Orionis	5·6	10 8	170
June 3	B.A.C. 4666 ...	6·5	8 3	206	29	68 Orionis	5·6	18 24	182
26	α Leonis	1·3	5 3	202					

JUPITER'S SATELLITES, 1906.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re.; *t*, Tr. Ing.; T, Tr. Egr.

January.

d	h	m	
1	9	41	i. *t*.
	9	46	ii. *t*.
	11	53	i. T.
	12	20	ii. T.
2	6	57	i. *o*.
	10	3	i. E.
3	4	8	i. *t*.
	4	18	ii. *o*.
	5	25	iii. *o*.
	6	20	i. T.
	7	10	iii. O.
	8	43	ii. E.
	9	16	iii. *e*.
	10	59	iii. E.
4	1	24	i. *o*.
	4	32	i. E.
	22	36	i. *t*.
	22	58	ii. *t*.
5	0	48	i. T.
	1	32	ii. T.
	19	51	i. *o*.
	23	1	i. E.
6	17	3	i. *t*.
	17	29	ii. *o*.
	19	15	i. T.
	19	20	iii. *t*.
	21	7	iii. T.
	22	1	ii. E.
7	14	18	i. *o*.
	17	30	i. E.
8	11	30	i. *t*.
	12	9	ii. *t*.
	13	42	i. T.
	14	44	ii. T.
9	8	45	i. *o*.
	11	59	i. E.
10	5	57	i. *t*.
	6	40	ii. *o*.
	8	10	i. T.
	8	57	iii. *o*.
	10	46	iii. O.
	11	18	ii. E.
	13	16	iii. *e*.
	15	1	iii. E.
11	3	13	i. *o*.
	6	27	i. E.
12	0	24	i. *t*.
	1	23	ii. *t*.
	2	37	i. T.
	3	57	ii. T.
	21	40	i. *o*.
13	0	56	i. E.
	18	*52*	*i. t.*
	19	*52*	*ii. o.*

Jan. (con.).

d	h	m	
13	21	4	i. T.
	22	53	iii. *t*.
14	0	36	ii. E.
	0	45	iii. T.
	16	7	i. *o*.
	19	25	i. E.
15	13	19	i. *t*.
	14	36	ii. *t*.
	15	32	i. T.
	17	10	ii. T.
16	10	35	i. *o*.
	13	54	i. E.
17	7	46	i. *t*.
	9	4	ii. *o*.
	9	59	i. T.
	12	34	iii. *o*.
	13	54	ii. E.
	14	27	iii. O.
	17	18	iii. *e*.
	19	3	iii. E.
18	5	3	i. *o*.
	8	23	i. E.
19	2	14	i. *t*.
	3	50	ii. *t*.
	4	27	i. T.
	6	25	ii. T.
	23	31	i. *o*.
20	2	52	i. E.
	20	42	i. *t*.
	22	17	ii. *o*.
	22	54	i. T.
21	2	33	iii. *t*.
	3	12	ii. E.
	4	27	iii. T.
	17	58	i. *o*.
	21	21	i. E.
22	15	9	i. *t*.
	17	4	ii. *t*.
	17	22	i. T.
	19	39	ii. T.
23	12	26	i. *o*.
	15	50	i. E.
24	9	37	i. *t*.
	11	31	ii. *o*.
	11	50	i. T.
	16	16	iii. *o*.
	16	30	ii. E.
	18	12	iii. O.
	21	19	iii. *e*.
	23	6	iii. E.
25	6	54	i. *o*.
	10	19	i. E.
26	*4*	*5*	*i. t.*
	6	*18*	*i. T.*

Jan. (con.).

d	h	m	
26	6	20	ii. *t*.
	8	56	ii. T.
27	1	22	i. *o*.
	4	48	i. E.
	22	33	i. *t*.
28	0	45	ii. *o*.
	0	46	i. T.
	5	48	ii. E.
	6	17	iii. *t*.
	8	14	iii. T.
	19	50	i. *o*.
	23	17	i. E.
29	17	1	i. *t*.
	19	14	i. T.
	19	36	ii. *t*.
	22	11	ii. T.
30	14	19	i. *o*.
	17	46	i. E.
31	11	29	i. *t*.
	13	42	i. T.
	14	0	ii. *o*.
	19	6	ii. E.
	20	4	iii. *o*.
	22	3	iii. O.

February.

d	h	m	
1	1	20	iii. *e*.
	3	9	iii. E.
	8	47	i. *o*.
	12	15	i. E.
2	5	57	i. *t*.
	8	11	i. T.
	8	52	ii. *t*.
	11	29	ii. T.
3	3	15	i. *o*.
	6	44	i. E.
4	0	26	i. *t*.
	2	39	i. T.
	3	16	ii. *o*.
	8	24	ii. E.
	10	6	iii. *t*.
	12	6	iii. T.
	21	44	i. *o*.
5	1	13	i. E.
	18	54	i. *t*.
	21	7	i. T.
	22	9	ii. *t*.
6	0	45	ii. T.
	16	12	i. *o*.
	19	42	i. E.
7	13	22	i. *t*.
	15	35	i. T.
	16	32	ii. *o*.

Feb. (con.).

d	h	m	
7	19	8	ii. O.
	19	10	ii. *e*.
	21	43	ii. E.
	23	56	iii. *o*.
8	1	58	iii. O.
	5	21	iii. *e*.
	7	11	iii. E.
	10	41	i. *o*.
	14	11	i. E.
9	7	51	i. *t*.
	10	4	i. T.
	11	27	ii. *t*.
	14	4	ii. T.
10	5	9	i. *o*.
	8	40	i. E.
11	2	19	i. *t*.
	4	33	i. T.
	5	48	ii. *o*.
	8	25	ii. O.
	8	28	ii. *e*.
	11	1	ii. E.
	14	0	iii. *t*.
	16	3	iii. T.
	23	38	i. *o*.
12	3	9	i. E.
	20	48	i. *t*.
	23	1	i. T.
13	0	45	ii. *t*.
	3	22	ii. T.
	18	7	i. *o*.
	21	38	i. E.
14	15	17	i. *t*.
	17	30	i. T.
	19	6	ii. *o*.
	21	43	ii. O.
	21	46	ii. *e*.
15	0	19	ii. E.
	3	53	iii. *o*.
	5	57	iii. O.
	9	22	iii. *e*.
	11	13	iii. E.
	12	36	i. *o*.
	16	7	i. E.
16	9	45	i. *t*.
	11	59	i. T.
	14	4	ii. *t*.
	16	41	ii. T.
17	7	4	i. *o*.
	10	36	i. E.
18	4	14	i. *t*.
	6	28	i. T.
	8	23	ii. *o*.
	11	1	ii. O.
	11	4	ii. *e*.

Feb. (con.).

d	h	m	
18	13	37	ii. E.
	17	59	iii. *t*.
	20	5	iii. T.
19	1	33	i. *o*.
	5	5	i. E.
	22	43	i. *t*.
20	0	56	i. T.
	3	23	ii. *t*.
	6	0	ii. T.
	20	2	i. *o*.
	23	34	i. E.
21	17	12	i. *t*.
	19	25	i. T.
	21	42	ii. *o*.
22	0	19	ii. O.
	0	22	ii. *e*.
	2	55	ii. E.
	7	54	iii. *o*.
	10	0	iii. O.
	13	23	iii. *e*.
	14	32	i. *o*.
	15	15	iii E.
	18	2	i. E.
23	11	41	i. *t*.
	13	54	i. T.
	16	43	ii. *t*.
	19	21	ii. T.
24	9	1	i. *o*.
	12	31	i. E.
25	6	10	i. *t*.
	8	24	i. T.
	11	0	ii. *o*.
	13	38	ii. O.
	13	40	ii. *e*.
	16	14	ii. E.
	22	3	iii. *t*.
26	0	10	iii. T.
	3	30	i. *o*.
	7	0	i. E.
27	0	39	i. *t*.
	2	53	i. T.
	6	3	ii. *t*.
	8	41	ii. T.
	21	59	i. *o*.
28	1	29	i. E.
	19	8	i. *t*.
	21	22	i. T.

March.

d	h	m	
1	0	20	ii. *o*.
	2	58	ii. O.
	2	58	ii. *e*.
	5	32	ii. E.

March

d	h	m
1	11	5
	14	
	16	2
	17	2
	19	1
	19	5
2	13	3
	15	5
	19	2
	22	
3	10	5
	14	2
4	8	
	10	2
	13	4
	18	5
5	2	1
	4	1
	5	2
	8	5
6	2	3
	4	5
	8	4
	11	2
	23	5
7	3	2
	21	
	23	2
8	3	
	8	
	16	
	18	1
	18	2
	21	2
	21	5
	23	2
9	15	3
	17	4
	22	
10	0	4
	12	5
	16	2
11	10	
	12	1
	16	2
	21	2
12	6	2
	7	2
	8	3
	10	5
13	4	3
	6	4
	11	2
	14	

arch (con.).

d	h	m	
	1	55	i. *o.*
	5	21	i. E.
	23	4	i. *t.*
	1	18	i. T.
	5	42	ii. *o.*
	10	45	ii. E.
	20	22	iii. *o.*
	20	25	i. *o.*
	22	33	iii. O.
	23	50	i. E.
	1	25	iii. *e.*
	3	22	iii. E.
	17	34	i. *t.*
	19	47	i. T.
	0	51	ii. *t.*
	3	30	ii. T.
	14	55	i. *o.*
	18	18	i. E.
	12	3	i. *t.*
	14	17	i. T.
	19	3	ii. *o.*
	0	4	ii. E.
	9	24	i. *o.*
	10	34	iii. *t.*
	12	46	iii. T.
	12	47	i. E.
	6	33	i. *t.*
	8	47	i. T.
	14	13	ii. *t.*
	16	52	ii. T.
	3	54	i. *o.*
	7	16	i. E.
	1	3	i. *t.*
	3	17	i. T.
	8	25	ii. *o.*
	13	22	ii. E.
	22	24	i. *o.*
	0	37	iii. *o.*
	1	45	i. E.
	2	50	iii. O.
	5	26	iii. *e.*
	7	25	iii. E.
	19	33	i. *t.*
	21	47	i. T.
4	3	36	ii. *t.*
	6	15	ii. T.
	16	54	i. *o.*
	20	14	i. E.
5	14	3	i. *t.*
	16	17	i. T.
	21	48	ii. *o.*
6	2	41	ii. E.
	11	24	i. *o.*
	14	43	i. E.
	14	51	iii. *t.*
	17	5	iii. T.
7	8	32	i. *t.*
	10	46	i. T.
	16	59	ii. *t.*
	19	38	ii. T.
8	5	54	i. *o.*
	9	12	i. E.
9	3	2	i. *t.*
	5	16	i. T.

March (con.).

d	h	m	
29	11	10	ii. *o.*
	15	59	ii. E.
30	0	24	i. *o.*
	3	41	i. E.
	4	55	iii. *o.*
	7	10	iii. O.
	9	27	iii. *e.*
	11	27	iii. E.
	21	32	i. *t.*
	23	46	i. T.
31	6	23	ii. *t.*
	9	2	ii. T.
	18	54	i. *o.*
	22	9	i. E.

April.

d	h	m	
1	16	2	i. *t.*
	18	16	i. T.
2	0	33	ii. *o.*
	5	18	ii. E.
	13	24	i. *o.*
	16	38	i. E.
	19	11	iii. *t.*
	21	26	iii. T.
3	10	32	i. *t.*
	12	46	i. T.
	19	45	ii. *t.*
	22	25	ii. T.
4	7	54	i. *o.*
	11	7	i. E.
5	5	2	i. *t.*
	7	16	i. T.
	13	57	ii. *o.*
	18	36	ii. E.
6	2	24	i. *o.*
	5	36	i. E.
	9	16	iii. *o.*
	11	32	iii. O.
	13	27	iii. *e.*
	15	28	iii. E.
	23	33	i. *t.*
7	1	46	i. T.
	9	10	ii. *t.*
	11	50	ii. T.
	20	54	i. *o.*
8	0	5	i. E.
	18	3	i. *t.*
	20	17	i. T.
9	3	20	ii. *o.*
	7	55	ii. E.
	15	24	i. *o.*
	18	34	i. E.
	23	33	iii. *t.*
10	1	50	iii. T.
	12	33	i. *t.*
	14	47	i. T.
	22	33	ii. *t.*
11	1	14	ii. T.
	9	54	i. *o.*
	13	2	i. E.
12	7	3	i. *t.*
	9	17	i. T.
	16	44	ii. *o.*

April (con.).

d	h	m	
12	21	14	ii. E.
13	4	25	i. *o.*
	7	31	i. E.
	13	39	iii. *o.*
	15	56	iii. O.
	17	28	iii. *e.*
	19	30	iii. E.
14	1	33	i. *t.*
	3	47	i. T.
	11	58	ii. *t.*
	14	38	ii. T.
	22	55	i. *o.*
15	2	0	i. E.
	20	4	i. *t.*
	22	18	i. T.
16	6	8	ii. *o.*
	10	32	ii. E.
	17	25	i. *o.*
	20	29	i. E.
17	3	57	iii. *t.*
	6	15	iii. T.
	14	33	i. *t.*
	16	48	i. T.
18	1	22	ii. *t.*
	4	3	ii. T.
	11	55	i. *o.*
	14	58	i E.
19	9	4	i. *t.*
	11	18	i. T.
	19	32	ii. *o.*
	23	51	ii. E.
20	6	26	i. *o.*
	9	26	i. E.
	18	3	iii. *o.*
	20	22	iii. O.
	21	28	iii. *e.*
	23	31	iii. E.
21	3	34	i. *t.*
	5	49	i. T.
	14	46	ii. *t.*
	17	27	ii. T.
22	0	56	i. *o.*
	3	55	i. E.
	22	5	i. *t.*
23	0	19	i. T.
	8	57	ii. *o.*
	13	9	ii. E.
	19	26	i. *o.*
	22	24	i. E.
24	8	22	iii. *t.*
	10	41	iii. T.
	16	35	i. *t.*
	18	49	i. T.
25	4	11	ii. *t.*
	6	52	ii. T.
	13	56	i. *o.*
	16	53	i. E.
26	11	5	i. *t.*
	13	20	i. T.
	22	22	ii. *o.*
27	2	28	ii. E.
	8	27	i. *o.*
	11	22	i. E.
	22	28	iii. *o.*

April (con.).

d	h	m	
28	0	49	iii. O.
	1	28	iii. *e.*
	3	33	iii. E.
	5	36	i. *t.*
	7	50	i. T.
	17	35	ii. *t.*
	20	17	ii. T.
29	2	57	i. *o.*
	5	50	i E.
30	0	6	i. *t.*
	2	20	i. T.
	11	47	ii. *o.*
	15	47	ii. E.
	21	27	i. *o.*

May.

d	h	m	
1	0	19	i. E.
	12	48	iii. *t.*
	15	9	iii. T.
	18	36	i. *t.*
	20	51	i. T.
2	7	0	ii. *t.*
	9	41	ii. T.
	15	58	i. *o.*
	18	48	i. E.
3	13	7	i. *t.*
	15	21	i. T.
4	1	12	ii. *o.*
	5	6	ii. E.
	10	28	i. *o.*
	13	17	i. E.
5	2	56	iii. *o.*
	5	18	iii. O.
	5	28	iii. *e.*
	7	35	iii. E.
	7	37	i. *t.*
	9	52	i. T.
	20	25	ii. *t.*
	23	6	ii. T.
6	4	58	i. *o.*
	7	45	i. E.
7	2	8	i. *t.*
	4	22	i. T.
	14	37	ii. *o.*
	18	24	ii. E.
	23	29	i. *o.*
8	2	14	i. E.
	17	15	iii. *t.*
	19	38	iii. T.
	20	38	i. *t.*
	22	53	i. T.
9	9	49	ii. *t.*
	12	31	ii. T.
	17	59	i. *o.*
	20	43	i. E.
10	15	9	i. *t.*
	17	23	i. T.
11	4	2	ii. *o.*
	7	43	ii. E.
	12	30	i. *o.*
	15	11	i. E.
12	7	24	iii. o.
	9	39	i. t.

May (con.).

d	h	m	
12	11	37	iii. E.
	11	54	i. T.
	23	14	ii. *t.*
13	1	56	ii. T.
	7	0	i. *o.*
	9	40	i. E.

July.

d	h	m	
9	0	32	ii. T.
	1	27	i. O.
	20	25	i. *t.*
	22	40	i. T.
10	15	22	ii. *e.*
	17	14	i. *e.*
	19	8	ii. O.
	19	57	i. O.
11	14	55	i. *t.*
	17	11	i. T.
12	9	33	iii. *t.*
	11	11	ii. *t.*
	11	42	i. *e.*
	12	12	iii. T.
	13	56	ii. T.
	14	27	i. O.
13	9	25	i. *t.*
	11	41	i. T.
14	4	41	ii. *e.*
	6	11	i. *e.*
	8	34	ii. O.
	8	57	i. O.
15	3	56	i. *t.*
	6	11	i. T.
	21	23	iii. *e.*
16	0	34	ii. *t.*
	0	39	i. *e.*
	2	18	iii. O.
	3	20	ii. T.
	3	27	i. O.
	22	26	i. *t.*
17	0	41	i. T.
	17	59	ii. *e.*
	19	7	i. *e.*
	21	57	i. O.
	21	59	ii. O.
18	16	56	i. *t.*
	19	12	i. T.
19	13	36	i. *e.*
	13	57	ii. *t.*
	14	0	iii. *t.*
	16	27	i. O.
	16	40	iii. T.
	16	43	ii. T.
20	11	26	i. *t.*
	13	42	i. T.
21	7	18	ii. *e.*
	8	4	i. *e.*
	10	58	i. O.
	11	25	ii. O.
22	5	57	i. *t.*
	8	12	i. T.
23	1	22	iii. e.
	2	33	i. e.
	3	21	ii. t.

July (c

d	h	m	
23	3	44	
	4	3	
	5	28	
	6	7	
	6	44	
24	0	27	
	2	42	
	20	36	
	21	1	
	23	58	
25	0	50	
	18	57	
	21	12	
26	15	30	
	16	44	
	18	25	
	18	28	
	19	30	
	21	8	
27	13	27	
	15	43	
28	9	55	
	9	58	
	12	58	
	14	15	
29	7	58	
	10	13	
30	4	26	
	5	20	i
	6	7	
	7	28	i
	7	44	i
	8	27	i
	8	53	i
	11	10	i
31	2	27	i
	4	43	i
	22	55	i
	23	13	i

Augus

d	h	m	
1	1	57	i
	3	40	i
	20	58	i
	23	13	i
2	17	23	i
	19	29	i
	20	27	i
	22	16	i
	22	49	i
3	1	33	i
	15	27	i
	17	43	i
4	11	52	i
	12	32	i
	14	57	i
	17	4	i
5	9	57	i
	12	13	i
6	6	20	i
	8	52	
	9	10	
	9	2	

Aug. (con.).

d	h	m	
6	11	39	ii. T.
	11	45	iii. E.
	12	49	iii. *o*.
	15	34	iii. O.
7	4	27	i. *t*.
	6	43	i. T.
8	0	48	i. *e*.
	1	50	ii. *e*.
	3	57	i. O.
	6	28	ii. O.
	22	57	i. *t*.
9	1	13	i. T.
	19	17	i. *e*.
	22	14	ii. *t*.
	22	27	i. O.
10	1	1	ii. T.
	3	10	iii. *t*.
	5	56	iii. T.
	17	27	i. *t*.
	19	43	i. T.
11	13	45	i. *e*.
	15	9	ii. *e*.
	16	57	i. O.
	19	53	ii. O.
12	11	57	i. *t*.
	14	13	i. T.
13	8	14	i. *e*.
	11	26	i. O.
	11	36	ii. *t*.
	13	18	iii. *e*.
	14	23	ii. T.
	15	45	iii. E.
	17	10	iii. *o*.
	19	57	iii. O.
14	6	27	i. *t*.
	8	43	i. T.
15	2	42	i. *e*.
	4	27	ii. *e*.
	5	56	i. O.
	9	16	ii. O.
16	0	57	i. *t*.
	3	13	i. T.
	21	10	i. *e*.
17	0	26	i. O.
	0	58	ii. *t*.
	3	45	ii. T.
	7	29	iii. *t*.
	10	17	iii. T.
	19	27	i. *t*.
	21	43	i. T.
18	15	39	i. *e*.
	17	46	ii. *e*.
	18	55	i. O.
	22	40	ii. O.
19	13	56	i. *t*.
	16	12	i. T.
20	10	7	i. *e*.
	13	25	i. O.
	14	20	ii. *t*.
	17	*7*	*ii. T.*
	17	*17*	*iii. e.*
	19	*46*	*iii. E.*
	21	*28*	*iii. o.*
	0	*17*	*iii. O.*

Aug. (con.).

d	h	m	
21	8	26	i. *t*.
	10	42	i. T.
22	4	36	i. *e*.
	7	3	ii. *e*.
	7	54	i. O.
	12	3	ii. O.
23	2	55	i. *t*.
	5	12	i. T.
	23	4	i. *e*.
24	2	24	i. O.
	3	41	ii. *t*.
	6	29	ii. T.
	11	47	iii. *t*.
	14	37	iii. T.
	21	25	i. *t*.
	23	41	i. T.
25	17	32	i. *e*.
	20	22	ii. *e*.
	20	53	i. O.
26	1	26	ii. O.
	15	55	i. *t*.
	18	11	i. T.
27	12	1	i. *e*.
	15	23	i. O.
	17	2	ii. *t*.
	19	50	ii. T.
	21	16	iii. *e*.
	23	45	iii. E.
28	1	44	iii. *o*.
	4	35	iii. O.
	10	24	i. *t*.
	12	40	i. T.
29	6	29	i. *e*.
	9	40	ii. *e*.
	9	52	i. O.
	14	48	ii. O.
30	4	54	i. *t*.
	7	10	i. T.
31	0	57	i. *e*.
	4	21	i. O.
	6	23	ii. *t*.
	9	11	ii. T.
	16	2	iii. *t*.
	18	54	iii. T.
	23	23	i. T.

September.

d	h	m	
1	1	39	i. T.
	19	26	i. *e*.
	22	51	i. O.
	22	59	ii. *e*.
2	4	10	ii. O.
	17	52	i. *t*.
	20	9	i. T.
3	13	54	i. *e*.
	17	20	i. O.
	19	43	ii. *t*.
	22	32	ii. T.
4	*1*	*14*	*iii. e.*
	3	*45*	*iii. E.*
	5	*58*	*iii. o.*
	8	*50*	*iii. O.*

Sept. (con.).

d	h	m	
4	12	22	i. *t*.
	14	38	i. T.
5	8	22	i. *e*.
	11	49	i. O.
	12	17	ii. *e*.
	17	32	ii. O.
6	6	51	i. *t*.
	9	7	i. T.
7	2	51	i. *e*.
	6	18	i. O.
	9	4	ii. *t*.
	11	52	ii. T.
	20	15	iii. *t*.
	23	8	iii. T.
8	1	20	i. *t*.
	3	37	i. T.
	21	19	i. *e*.
9	0	47	i. O.
	1	35	ii. *e*.
	6	53	ii. O.
	19	49	i. *t*.
	22	6	i. T.
10	15	47	i. *e*.
	19	16	i. O.
	22	23	ii. *t*.
11	1	12	ii. T.
	5	12	iii. *e*.
	7	44	iii. E.
	10	8	iii. *o*.
	13	1	iii. O.
	14	19	i. *t*.
	16	35	i. T.
12	10	16	i. *e*.
	13	45	i. O.
	14	53	ii. *e*.
	20	14	ii. O.
13	8	48	i. *t*.
	11	4	i. T.
14	4	44	i. *e*.
	8	14	i. O.
	11	43	ii. *t*.
	14	31	ii. T.
15	0	25	iii. *t*.
	3	17	i. *t*.
	3	19	iii. T.
	5	32	i. T.
	23	12	i. *e*.
16	2	43	i. O.
	4	11	ii. *e*.
	9	35	ii. O.
	21	46	i. *t*.
17	0	2	i. T.
	17	41	i. *e*.
	21	12	i. O.
18	1	2	ii. *t*.
	3	50	ii. T.
	9	10	iii. *e*.
	11	44	iii. E
	14	15	iii. *o*.
	16	15	i. *t*.
	17	10	iii. O.
	18	31	i. T.
19	12	9	i. *e*.
	15	41	i. O.

Sept. (con.).

d	h	m	
19	17	29	ii. *e*.
	22	55	ii. O.
20	10	43	i. *t*.
	13	0	i. T.
21	6	37	i. *e*.
	10	10	i. O.
	14	20	ii. *t*.
	17	9	ii. T.
22	4	30	iii. *t*.
	5	12	i. *t*.
	7	26	iii. T.
	7	29	i. T.
23	1	6	i. *e*.
	4	38	i. O.
	6	48	ii. *e*.
	12	14	ii. O.
	23	41	i. *t*.
24	1	58	i. T.
	19	34	i. *e*.
	23	7	i. O.
25	3	38	ii. *t*.
	6	27	ii. T.
	13	9	iii. *e*.
	15	44	iii. E.
	18	10	i. *t*.
	18	19	iii *o*.
	20	26	i. T.
	21	15	iii. O.
26	14	2	i. *e*.
	17	36	i. O.
	20	5	ii. *e*.
27	1	33	ii. O.
	12	38	i. *t*.
	14	55	i. T.
28	8	31	i. *e*.
	12	4	i. O.
	16	56	ii. *t*.
	19	45	ii. T.
29	7	7	i. *t*.
	8	32	iii. *t*.
	9	23	i. T.
	11	29	iii. T.
30	2	59	i. *e*.
	6	33	ii. O.
	9	24	ii. *e*.
	14	51	ii O.

October.

d	h	m	
1	1	35	i. *t*.
	3	52	i. T.
	21	27	i. *e*.
2	1	1	i. O.
	6	14	ii. *t*.
	9	3	ii. T.
	17	8	iii. *e*.
	19	44	iii. E.
	20	4	i. *t*.
	22	19	iii. *o*.
	22	21	i. T.
3	1	17	iii. O.
	15	56	i. *e*.
	19	29	i. O.
	22	41	ii. *e*.

Oct. (con.).

d	h	m	
4	4	9	ii. O.
	14	32	i. *t*.
	16	49	i. T.
5	10	24	i. *e*.
	13	58	i. O.
	19	31	ii. *t*.
	22	20	ii. T.
6	9	1	i. *t*.
	11	17	i. T.
	12	30	iii. *t*.
	15	28	iii. T.
7	4	53	i. *e*.
	8	26	i. O.
	11	59	ii. *e*.
	17	26	ii. O.
8	2	59	iv. *t*.
	3	29	i. *t*.
	3	46	iv. T.
	5	46	i. T.
	23	21	i. *e*.
9	2	54	i. O.
	8	47	ii. *t*.
	11	36	ii. T.
	21	6	iii. *e*.
	21	57	i. *t*.
	23	44	iii. E.
10	0	14	i. T.
	2	16	iii. *o*.
	5	14	iii. O.
	17	49	i. *e*.
	21	22	i. O.
11	1	17	ii. *e*.
	6	43	ii. O.
	16	25	i. *t*.
	18	42	i. T.
12	12	18	i. *e*.
	15	50	i. O.
	22	3	ii. *t*.
13	0	53	ii. T.
	10	53	i. *t*.
	13	10	i. T.
	16	24	iii. *t*.
	19	22	iii. T.
14	6	46	i. *e*.
	10	18	i. O.
	14	35	ii. *e*
	19	59	ii. O.
15	5	21	i. *t*.
	7	38	i. T.
16	1	14	i. *e*.
	4	46	i. O.
	11	18	ii. *t*.
	11	31	iv. *o*.
	12	5	iv. O.
	14	8	ii. T.
	23	49	i. *t*.
17	1	5	iii. *e*.
	2	6	i. T.
	3	44	iii. E.
	6	7	iii. *o*.
	9	6	iii. O.
	19	43	i. *e*.
	23	14	i. O.
18	3	52	ii. *e*.

Oct. (co

d	h	m
18	9	15
	18	17
	20	34
19	14	11
	17	42
20	0	33
	3	23
	12	45
	15	2
	20	14
	23	13
21	8	39
	12	9
	17	10
	22	30
22	7	13
	9	30
23	3	8
	6	37
	13	48
	16	37
24	1	40
	3	57
	5	3
	7	43
	9	54
	12	54
	20	6
	21	11
	21	36
25	1	5
	6	28
	11	44
	20	8
	22	25
26	16	4
	19	32
27	3	2
	5	51
	14	35
	16	52
	23	59
28	2	59
	10	33
	13	59
	19	46
29	0	58
	9	2
	11	19
30	5	1
	8	27
	16	15
	19	5
31	3	30
	5	47
	9	1
	11	43
	13	36
	16	36
	23	30

Novemb

d	h	m
1	2	54

Nov. (con.).				*Nov.* (con.).				*Nov.* (con).				*Dec.* (con.).				*Dec.* (con.).				*Dec.* (con			
d	h	m		d	h	m		d	h	m		d	h	m		d	h	m		d	h	m	
1	9	3	ii. *e.*	11	10	15	iii. T.	21	20	57	iii. *e.*	1	4	28	i. O.	11	16	25	i. *e.*	22	0	30	i
	14	11	ii. O.		14	20	i. *e.*		23	43	iii. E.		14	51	ii. *t.*		19	5	i. O.		0	45	i
	21	57	i. *t.*		17	36	i. O.	22	0	16	iii. *o.*		17	41	ii. T.	12	6	14	ii. *t.*		2	2	i
2	0	14	i. T.	12	0	56	ii. *e.*		3	17	iii. O.		23	30	i. *t.*		9	4	ii. T.		7	16	i
	4	8	iv. *o.*		5	46	ii. O.		5	11	i. *e.*	2	1	47	i. T.		14	6	i. *t.*		9	40	i
	5	4	iv. O.		12	39	i. *t.*		8	16	i O.		17	34	iii. *t.*		16	23	i. T.		21	37	i
	17	58	i. *e.*		14	56	i. T.		16	49	ii. *e.*		20	2	i. *e.*	13	8	54	iii. *e.*	23	0	27	i
	21	21	i. O.	13	8	49	i. *e.*		21	16	ii. O.		20	34	iii. T.		10	53	i. *e.*		4	41	i
3	5	28	ii. *t.*		12	3	i. O.	23	3	19	i. *t.*		22	54	i. O.		13	20	iii. O.		6	58	i
	8	17	ii. T.		21	2	ii. *t.*		5	36	i. T.	3	8	41	ii. *e.*		13	31	i. O.	24	1	45	i
	16	24	i. *t.*		23	52	ii. T		23	39	i. *e.*		12	42	ii. O.		18	1	iv. *t.*		3	26	i
	18	41	i. T.	14	7	6	i. *t.*	24	2	43	i. O.		17	56	i. *t.*		19	28	iv. T.		4	6	i
4	3	39	iii. *t.*		9	23	i. T.		12	33	ii. *t.*		20	13	i. T.	14	0	34	ii. *e.*		6	26	i
	6	40	iii. T.		16	58	iii. *e.*		15	23	ii. T.	4	14	30	i. *e.*		4	4	ii. O.		16	26	i
	12	26	i. *e.*		19	43	iii. E.		21	45	i. *t.*		17	20	i. O.		8	32	i. *t.*		19	24	i
	15	48	i. O.		20	47	iii. *o.*	25	0	2	i. T.	5	3	59	ii. *t.*		10	49	i. T.		23	7	i
	22	21	ii. *e.*		23	48	iii. O.		14	11	iii. *t.*		5	24	iv. *e.*	15	5	22	i. *e.*	25	1	24	i
5	3	23	ii. O.	15	3	17	i. *e.*		17	12	iii. T.		6	6	iv. E.		7	56	i. O.		20	13	i
	10	51	i. *t.*		6	30	i. O.		18	8	i. *e.*		6	49	ii. T.		19	22	ii. *t.*		22	32	i
	13	8	i. T.		14	14	ii. *e.*		21	9	i. O.		10	35	iv. *o.*		22	12	ii. T.	26	10	44	i
6	6	55	i. *e.*		18	57	ii. O.	26	6	6	ii. *e.*		11	47	iv. O.	16	2	58	i. *t.*		13	33	i
	10	15	i. O.	16	1	33	i. *t.*		10	25	ii. O.		12	23	i. *t.*		5	15	i. T.		17	33	i
	18	40	ii. *t.*		3	50	i. T.		16	12	i. *t.*		14	40	i. T.		23	50	i. *e.*		19	50	i
	21	30	ii. T.		21	45	i. *e.*		18	29	i. T.	6	4	55	iii. *e.*	17	0	10	iii. *t.*	27	14	41	i
7	5	18	i. *t.*	17	0	57	i. O.	27	3	33	iv. *t.*		8	59	i. *e.*		2	22	i. O.		16	50	i
	7	35	i. T.		10	13	ii. *t.*		4	55	iv. T.		10	2	iii. O.		3	11	iii. T.		16	58	i
	12	59	iii. *e.*		13	3	ii. T.		12	36	i. *e.*		11	46	i. O.		13	51	ii. *e.*		19	50	i
	15	42	iii. E.		19	59	i. *t.*		15	35	i. O.		21	59	ii. *e.*		17	11	ii. O.	28	5	40	i
	17	14	iii. *o.*		22	16	i. T.	28	1	42	ii. *t.*	7	1	49	ii. O.		21	24	i. *t.*		8	30	i
	20	14	iii. O.	18	10	45	iii. *t.*		4	32	ii. T.		6	49	i. *t.*		23	41	i. T.		11	59	i
8	1	23	i. *e.*		13	46	iii. T.		10	38	i *t.*		9	6	i. T.	18	18	19	i. *e.*		14	16	i
	4	42	i. O.		16	14	i. *e.*		12	55	i. T.	8	3	27	i. *e.*		20	48	i. O.	29	9	7	i
	11	39	ii. *e.*		19	23	i. O.	29	0	56	iii. *e.*		6	13	i. O.	19	8	29	ii. *t.*		11	24	i
	16	35	ii. O.		19	48	iv. *o.*		6	41	iii. O.		17	7	ii. *t.*		11	19	ii. T.		23	51	i
	23	45	i. *t.*		20	55	iv. O.		7	5	i. *e.*		19	57	ii. T.		15	50	i. *t.*	30	2	41	i
9	2	2	i. T.	19	3	31	ii. *e.*		10	2	i. O.	9	1	14	i. *t.*		18	7	i. T.		6	24	i
	19	52	i. *e.*		8	7	ii. O.		19	24	ii. *e.*		3	31	i. T.	20	12	48	i. *e.*		8	6	i
	23	9	i. O.		14	26	i. *t.*		23	34	ii. O.		20	53	iii. *t.*		12	53	iii. *e.*		8	41	i
10	7	51	ii. *t.*		16	43	i. T.	30	5	4	i. *t.*		21	56	i. *e.*		15	14	i. O.		9	37	i
	10	41	ii. T.	20	10	42	i. *e.*		7	21	i. T.		23	54	iii. T.		16	35	iii. O.	31	3	33	i
	12	17	iv. *t.*		13	50	i. O.					10	0	39	i. O.	21	3	9	ii. *e.*		5	52	i
	13	33	iv. T.		23	23	ii. *t.*						11	16	ii. *e.*		6	17	ii. O.		6	41	i
	18	12	i. *t.*	21	2	13	ii. T.	*December.*					14	57	ii. O.		10	16	i. *t.*		9	41	i
	20	29	i. T.		8	52	i. *t.*						19	41	i. *t.*		12	33	i. T.		18	47	i
1	7	15	iii. *t.*		11	9	i. T.	1	1	33	i. *e.*		21	57	i. T.		23	15	iv. *e.*		21	45	i

Satellites I., II., and III. *reappear* after eclipse, and III. *disappears* on the *f* side fro January to May. Satellites I., II., and III. *disappear*, and III. *reappears* on the *p* side from Ju to December.

Satellite IV. *disappears* and *reappears* on the *p* side near the north limb in December.

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Jan. 10h	Feb. 9½h	March. 9h	April. 9h	May. 8½h	July. 15h	Aug. 14½h	Sept. 14h	Oct. 14h	Nov. 13h	Dec. 12h	Day.
1	♃♃43O	4O32●	13O24	2O134	O4312		O4123	321O4	31O24	4O13●	4O213	1
2	432O●	♃41O3	O1243	1O234	♃31O4		41O23	3O124	32O14	1O423	421O3	2
3	14O2●	42O13	214O3	3O124	32O14		423O1	3O24●	21O34	2O134	43O1●	3
4	O1423	41O32	♃42O3	32O4●	31O24		43O●●	♃2O43	O1234	23O4●	341O2	4
5	21O34	34O12	43O12	321O4	♃O324		431O2	4O13●	O1234	♃3O24	32O14	5
6	2O134	321O4	♃431O	43O12	2O134		42O1●	41O23	♃21O4	3O124	2O134	6
7	31O24	32O14	432O1	41O23	12O34		421O3	42O31	3O41●	21O4●	1O234	7
8	3O124	1O324	413O2	42O13	O3124		4O123	4321O	314O2	O134●	O2134	8
9	32O4●	♃O234	4O123	41O23	31O24	2O134	41O23	43O12	432O1	1O243	21O34	9
10	1O4●●	2O134	421O3	43O12	324O1	12O34	23O41	431O2	421O3	24O13	3O14●	10
11	O1423	1O34●	24O13	4321O	431O2	♃O234	32O4●	42O13	4O123	4213O	31O24	11
12	124O3	3O124	3O42●	♃432O	4O12●	32O14	31O24	42O13	4O23●	♃43O2	32O14	12
13	42O13	312O4	31O24	43O12	42O3●	321O4	2O14●	1O423	421O3	43O12	2O4●●	13
14	431O2	32O41	32O14	1O423		3O124	21O34	♃O314	432O1	4231O	41O23	14
15	43O12	41O2●	31O4●	2O143		13O42	O2134	231O4	341O2	42O13	4O123	15
16	4321O	4O123	O1324	1O34●		24O13	1O234	3O214	♃3O41	41O23	421O3	16
17	431O●	42O3●	12O34	3O124		412O3	23O14	31O24	21O34	♃4O13	432O1	17
18	4O123	41O3●	2O134	321O4		4O123	321O4	23O14	O1234	♃214O	431O2	18
19	412O3	43O12	1O342	32O14		♃♃4O●	♃34O2	2O34●	1O234	3O124	432O1	19
20	2O413	4312O	341O2	3O24●		4321O	♃43O1	1O423	♃2O34	3O24●	4213O	20
21	13O24	432O1	342O1	1O234		43O21	421O3	4O213	32O14	231O4	♃4O23	21
22	3O124	14O2●	431O●	2O413		431O2	4O213	4213O	31O24	2O134	O1423	22
23	321O4	O1243	4O312	41O3●		42O31	41O23	43O21	♃3O14	1O234	21O34	23
24	♃32O4	2O43●	412O3	♃4O12		12O43	♃42O1	431O2	21O34	♃O134	32O14	24
25	O324●	12O34	42O13	4312O		O1243	4321O	42O1●	4O213	21O34	31O24	25
26	12O34	3O124	41O32	432O1		1O234	34O12	421O3	41O23	3O214	♃3O14	26
27	2O134	312O4	♃43O2	43O2●		♃23O4	3O24●	♃4O23	42O13	34O2●	231O4	27
28	13O42	32O14	324O●	41O32		3O214	21O34	4O123	432O1	4321O	♃O234	28
29	34O12		312O4	42O13		31O24	O134●	2134O	431O2	42O13	O1243	29
30	4321O		O3124	412O3		2O314	1O234	3O14●	43O21	41O23	214O3	30
31	432O1		♃1O34			21O43	2O314		421O●		423O1	31

The circle (O) represents Jupiter; ♃ signifies that the satellite is on the disk; ● signifies that the satellite is behind the disk or in the shadow.

Ephemeris of Jupiter's Satellite VI.

At Greenwich Mean Noon.

	Pos.-angle.	Dist.		Pos.-angle.	Dist.		Pos.-angle.	Dist.
	°	′		°	′		°	′
Jan. 2	118	50	Feb. 6	97	59	Mar. 13	79	43
7	114	53	11	94	58	18	75	39
12	111	56	16	92	57	23	71	35
17	108	58	21	90	55	28	65	31
22	105	59	26	88	53	Apr. 2	60	27
27	102	60	Mar. 3	85	50	7	55	22
Feb. 1	99	60	8	82	47	12	49	17

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49·3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 46·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 19·3	67	2 15·2	77	4 4·9		

SATELLITES OF SATURN.

East Elongations.

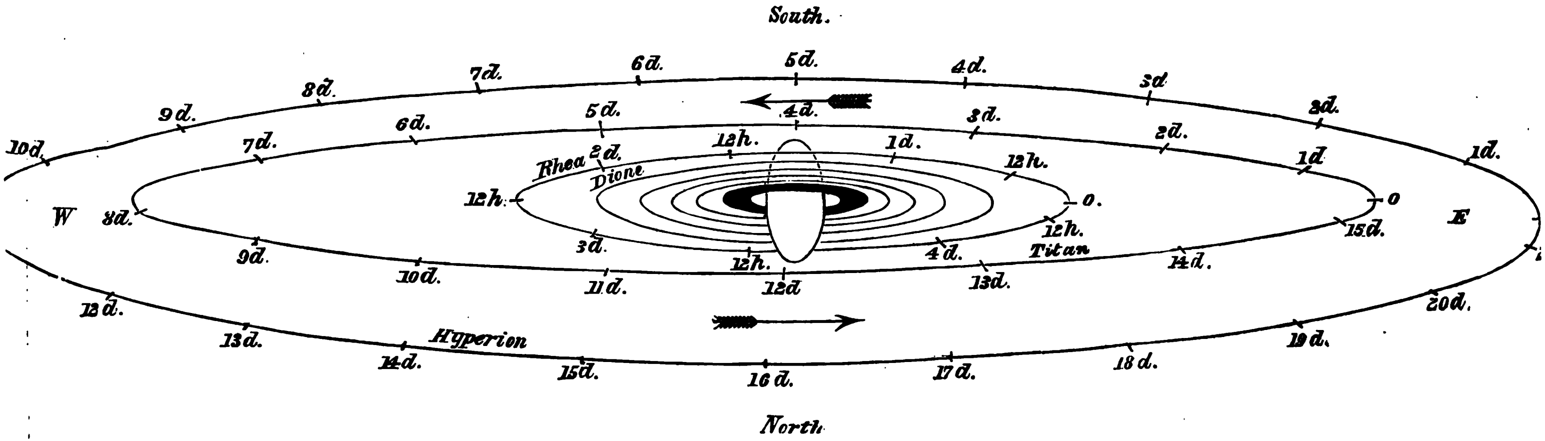
MIMAS.

April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
26	22·5	1	15·6	1	18·1	1	21·8	1	1·5	1	3·7	1	7·4	1	9·8	1	13·6
27	22·1	2	14·2	2	16·7	2	20·4	2	0·1	2	2·3	2	6·0	2	8·4	2	12·2
28	19·8	3	12·9	3	15·3	3	19·1	2	22·7	3	1·0	3	4·6	3	7·0	3	10·9
29	18·4	4	11·5	4	13·9	4	17·7	3	21·3	3	23·6	4	3·2	4	5·6	4	9·5
30	17·0	5	10·1	5	12·6	5	16·3	4	19·9	4	22·2	5	1·9	5	4·3	5	8·1
		6	8·7	6	11·2	6	14·9	5	18·6	5	20·8	6	0·5	6	2·9	6	6·7
		7	7·3	7	9·8	7	13·5	6	17·2	6	19·4	6	23·1	7	1·5	7	5·3
		8	6·0	8	8·4	8	12·1	7	15·8	7	18·0	7	21·7	8	0·1	8	4·0
		9	4·6	9	7·0	9	10·7	8	14·4	8	16·6	8	20·3	8	22·7	9	2·6
		10	3·2	10	5·6	10	9·4	9	13·0	9	15·3	9	18·9	9	21·3	10	1·2
		11	1·8	11	4·3	11	8·0	10	11·6	10	13·9	10	17·5	10	20·0	10	23·8
		12	0·5	12	2·9	12	6·6	11	10·2	11	12·5	11	16·2	11	18·6	11	22·4
		12	23·1	13	1·5	13	5·2	12	8·8	12	11·1	12	14·8	12	17·2	12	21·1
		13	21·7	14	0·1	14	3·8	13	7·5	13	9·7	13	13·4	13	15·8	13	19·7
		14	20·3	14	22·7	15	2·4	14	6·1	14	8·3	14	12·0	14	14·5	14	18·3
		15	18·9	15	21·3	16	1·0	15	4·7	15	6·9	15	10·6	15	13·1	15	17·0
		16	17·6	16	20·0	16	23·7	16	3·3	16	5·6	16	9·2	16	11·7	16	15·6
		17	16·2	17	18·6	17	22·3	17	1·9	17	4·2	17	7·8	17	10·3	17	14·2
		18	14·8	18	17·2	18	20·9	18	0·5	18	2·8	18	6·5	18	8·9	18	12·8
		19	13·4	19	15·8	19	19·5	18	23·1	19	1·4	19	5·1	19	7·6	19	11·5
		20	12·0	20	14·4	20	18·1	19	21·8	20	0·0	20	3·7	20	6·2	20	10·1
		21	10·7	21	13·1	21	16·7	20	20·4	20	22·6	21	2·3	21	4·8	21	8·7
		22	9·3	22	11·7	22	15·3	21	19·0	21	21·2	22	1·0	22	3·4	22	7·3
		23	7·9	23	10·3	23	14·0	22	17·6	22	19·8	22	23·6	23	2·0	23	6·0
		24	6·5	24	8·9	24	12·6	23	16·2	23	18·5	23	22·2	24	0·6	24	4·6
		25	5·2	25	7·5	25	11·2	24	14·8	24	17·1	24	20·8	24	23·3	25	3·2
		26	3·8	26	6·1	26	9·8	25	13·4	25	15·7	25	19·4	25	21·9	26	1·8
		27	2·4	27	4·7	27	8·4	26	12·0	26	14·3	26	18·1	26	20·5	27	0·5
		28	1·0	28	3·4	28	7·0	27	10·7	27	12·9	27	16·7	27	19·1	27	23·1
		28	23·6	29	2·0	29	5·6	28	9·3	28	11·5	28	15·3	28	17·8	28	21·7
		29	22·2	30	0·6	30	4·3	29	7·9	29	10·2	29	13·9	29	16·4	29	20·3
		30	20·8	30	23·2	31	2·9	30	6·5	30	8·8	30	12·5	30	15·0	30	18·9
		31	19·5					31	5·1			31	11·2			31	17·6

ENCELADUS.

April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
26	1·7	1	13·3	2	1·8	2	5·2	1	8·5	1	20·7	1	23·9	1	3·4	1	6·9
27	10·6	2	22·2	3	10·7	3	14·1	2	17·4	3	5·6	3	8·8	2	12·3	2	15·8
28	19·5	4	7·1	4	19·5	4	23·0	4	2·3	4	14·4	4	17·7	3	21·1	4	0·6
30	4·4	5	16·0	6	4·4	6	7·9	5	11·2	5	23·3	6	2·5	5	6·0	5	9·5
		7	0·9	7	13·3	7	16·7	6	20·0	7	8·2	7	11·4	6	14·9	6	18·4
		8	9·8	8	22·2	9	1·6	8	4·9	8	17·0	8	20·3	7	23·8	8	3·3
		9	18·7	10	7·1	10	10·5	9	13·8	10	1·9	10	5·2	9	8·7	9	12·2
		11	3·5	11	16·0	11	19·4	10	22·7	11	10·8	11	14·1	10	17·6	10	21·1
		12	12·4	13	0·8	13	4·2	12	7·5	12	19·7	12	22·9	12	2·4	12	6·0
		13	21·3	14	9·7	14	13·1	13	16·4	14	4·5	14	7·8	13	11·3	13	14·9
		15	6·2	15	18·6	15	22·0	15	1·3	15	13·4	15	16·7	14	20·2	14	23·8
		16	15·1	17	3·5	17	6·9	16	10·2	16	22·3	17	1·6	16	5·1	16	8·7
		18	0·0	18	12·4	18	15·8	17	19·0	18	7·2	18	10·5	17	14·0	17	17·6
		19	8·9	19	21·3	20	0·6	19	3·9	19	16·1	19	19·4	18	22·9	19	2·5
		20	17·8	21	6·2	21	9·5	20	12·8	21	1·0	21	4·3	20	7·7	20	11·4
		22	2·7	22	15·0	22	18·4	21	21·7	22	9·8	22	13·2	21	16·6	21	20·3
		23	11·6	23	23·9	24	3·3	23	6·5	23	18·7	23	22·1	23	1·5	23	5·2
		24	20·4	25	8·8	25	12·2	24	15·4	25	3·6	25	7·0	24	10·4	24	14·1
		26	5·3	26	17·7	26	21·0	26	0·3	26	12·4	26	15·8	25	19·3	25	23·0
		27	14·2	28	2·6	28	5·9	27	9·2	27	21·3	28	0·7	27	4·2	27	7·9
		28	23·1	29	11·4	29	14·8	28	18·0	29	6·2	29	9·6	28	13·1	28	16·8
		30	8·0	30	20·3	30	23·7	30	2·9	30	15·1	30	18·5	29	22·0	30	1·7
		31	16·9					31	11·8							31	10·6

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

(The Vertical Scale is twice the Horizontal.)

East Elongations.

TETHYS.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
26 8·8	2 0·7	1 5·8	1 10·7	2 12·7	1 17·3	1 22·0	1 2·8	1 7·8
28 6·1	3 22·1	3 3·1	3 8·0	4 10·0	3 14·6	3 19·3	3 0·1	3 5·1
30 3·4	5 19·4	5 0·5	5 5·3	6 7·3	5 11·9	5 16·6	4 21·4	5 2·4
	7 16·7	6 21·8	7 2·6	8 4·6	7 9·2	7 13·9	6 18·7	6 23·7
	9 14·0	8 19·1	8 23·9	10 1·9	9 6·5	9 11·2	8 16·0	8 21·0
	11 11·3	10 16·4	10 21·2	11 23·2	11 3·8	11 8·5	10 13·3	10 18·3
	13 8·6	12 13·7	12 18·5	13 20·5	13 1·1	13 5·8	12 10·6	12 15·7
	15 6·0	14 11·0	14 15·8	15 17·8	14 22·4	15 3·1	14 7·9	14 13·0
	17 3·3	16 8·3	16 13·1	17 15·0	16 19·7	17 0·4	16 5·2	16 10·3
	19 0·6	18 5·6	18 10·4	19 12·3	18 16·9	18 21·7	18 2·6	18 7·6
	20 21·9	20 2·9	20 7·7	21 9·6	20 14·2	20 19·0	19 23·9	20 5·0
	22 19·2	22 0·2	22 5·0	23 6·9	22 11·5	22 16·3	21 21·2	22 2·3
	24 16·6	23 21·5	24 2·3	25 4·2	24 8·8	24 13·6	23 18·5	23 23·6
	26 13·9	25 18·8	25 23·6	27 1·5	26 6·1	26 10·9	25 15·8	25 21·0
	28 11·2	27 16·1	27 20·9	28 22·8	28 3·4	28 8·2	27 13·1	27 18·3
	30 8·5	29 13·4	29 18·2	30 20·1	30 0·7	30 5·5	29 10·4	29 15·6
			31 15·5					31 12·9

DIONE.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
28 2·7	3 14·2	2 17·0	2 19·5	1 21·9	1 0·0	1 2·1	2 22·1	3 0·7
30 20·5	6 7·9	5 10·7	5 13·2	4 15·5	3 17·6	3 19·8	5 15·8	5 18·5
	9 1·6	8 4·4	8 6·9	7 9·2	6 11·3	6 13·4	8 9·5	8 12·2
	11 19·4	10 22·1	11 0·6	10 2·8	9 4·9	9 7·1	11 3·2	11 5·9
	14 13·1	13 15·8	13 18·2	12 20·5	11 22·6	12 0·7	13 20·9	13 23·6
	17 6·8	16 9·5	16 11·9	15 14·1	14 16·2	14 18·4	16 14·5	16 17·3
	20 0·5	19 3·1	19 5·6	18 7·7	17 9·9	17 12·1	19 8·2	19 11·1
	22 18·2	21 20·8	21 23·2	21 1·4	20 3·5	20 5·7	22 1·9	22 4·8
	25 11·9	24 14·5	24 16·9	23 19·0	22 21·1	22 23·4	24 19·6	24 22·5
	28 5·6	27 8·2	27 10·5	26 12·7	25 14·8	25 17·1	27 13·3	27 16·2
	30 23·3	30 1·9	30 4·2	29 6·3	28 8·5	28 10·8	30 7·0	30 9·9
						31 4·4		

RHEA.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
27 3·6	1 16·1	2 7·6	3 22·6	4 13·1	5 3·3	2 5·2	2 19·7	4 10·8
	6 4·6	6 20·0	8 10·9	9 1·4	9 15·6	6 17·5	7 8·1	8 23·3
	10 17·1	11 8·5	12 23·3	13 13·7	14 3·9	11 5·9	11 20·6	13 11·8
	15 5·6	15 20·9	17 11·7	18 2·0	18 16·2	15 18·2	16 9·0	18 0·3
	19 18·1	20 9·3	22 0·0	22 14·3	23 4·5	20 6·6	20 21·4	22 12·8
	24 6·6	24 21·8	26 12·4	27 2·6	27 16·8	24 19·0	25 9·9	27 1·3
	28 19·1	29 10·2	31 0·7	31 14·9		29 7·3	29 22·3	31 13·9

TITAN.

May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h
8 8·3	9 7·4	11 4·8	12 0·2	12 19·0	14 14·1	15 10·7	1 9·8
24 8·1	25 6·3	27 2·6	27 21·7	28 16·5	30 12·2		17 9·4

East Elongations.

HYPERION.

May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h
2 3·1 23 9·6	13 15·1	4 19·7 25 22·8	16 1·2	6 3·6 27 6·2	18 9·4	8 14·2 29 21·1	21 5·5

IAPETUS.

	d h	d h	d h	d h
E. Elongation	Apr. 20 4·6,	July 9 6·2,	Sept. 25 9·6,	Dec. 13 13·9
Inf. Conjunction ...	May 10 15·8,	July 29 4·1,	Oct. 15 7·4	
W. Elongation	May 31 5·3,	Aug. 18 3·1,	Nov. 4 9·1	
Sup. Conjunction ...	June 19 22·1,	Sept. 6 8·4,	Nov. 23 23·3	

APPARENT ELEMENTS OF SATURN'S RINGS.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 19	6 17·6	36·50	5·92	9 20·0 N.	8 0·5 N.
Feb. 8	6 9·2	36·05	5·19	8 16·8 N.	7 43·9 N.
28	5 59·8	35·96	4·49	7 10·3 N.	7 27·1 N.
Mar. 20	5 50·0	36·18	3·83	6 4·8 N.	7 10·3 N.
April 9	5 40·8	36·74	3·25	5 4·5 N.	6 53·5 N.
29	5 32·6	37·61	2·77	4 13·7 N.	6 36·6 N.
May 19	5 26·2	38·75	2·43	3 35·9 N.	6 19·6 N.
June 8	5 22·0	40·07	2·27	3 14·4 N.	6 2·5 N.
28	5 20·7	41·47	2·31	3 11·2 N.	5 45·4 N.
July 18	5 22·2	42·79	2·57	3 26·3 N.	5 28·3 N.
Aug. 7	5 26·4	43·82	3·02	3 57·4 N.	5 11·1 N.
27	5 32·5	44·37	3·59	4 38·9 N.	4 53·8 N.
Sept. 16	5 39·0	44·33	4·15	5 22·5 N.	4 36·5 N.
Oct. 6	5 44·7	43·71	4·56	5 59·6 N.	4 19·2 N.
26	5 48·4	42·62	4·73	6 22·6 N.	4 1·8 N.
Nov. 15	5 49·4	41·27	4·64	6 27·2 N.	3 44·4 N.
Dec. 5	5 47·7	39·87	4·30	6 11·7 N.	3 26·9 N.
25	5 43·2	38·59	3·79	5 38·0 N.	3 9·4 N.

SATELLITES OF URANUS.

North Elongations.

ARIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
16	18·1	1	21·1	1	3·0	1	9·0	3	3·7	2	9·7	2	15·6
19	6·6	4	9·6	3	15·5	3	21·5	5	16·2	4	22·2	5	4·1
21	19·1	6	22·1	6	4·0	6	10·0	8	4·7	7	10·7	7	16·6
24	7·6	9	10·5	8	16·5	8	22·6	10	17·2	9	23·2	10	5·1
26	20·1	11	23·0	11	5·0	11	11·1	13	5·7	12	11·7	12	17·6
29	8·6	14	11·5	13	17·5	13	23·6	15	18·2	15	0·2	15	6·1
		17	0·0	16	6·0	16	12·1	18	6·7	17	12·7		
		19	12·5	18	18·5	19	0·6	20	19·2	20	1·2		
		22	1·0	21	7·0	21	13·1	23	7·7	22	13·7		
		24	13·5	23	19·5	24	1·6	25	20·2	25	2·2		
		27	2·0	26	8·0	26	14·1	28	8·7	27	14·7		
		29	14·5	28	20·5	29	2·6	30	21·2	30	3·1		
						31	15·2						

UMBRIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
11	15·4	2	8·7	4	12·6	3	13·1	1	13·7	3	17·7	2	17·9
15	18·9	6	12·2	8	16·1	7	16·6	5	17·2	7	21·1	6	21·4
19	22·3	10.	15·7	12	19·6	11	20·1	9	20·7	12	0·6	11	0·8
24	1·8	14	19·1	16	23·1	15	23·6	14	0·2	16	4·1	15	4·3
28	5·3	18	22·6	21	2·6	20	3·2	18	3·7	20	7·6	19	7·7
		23	2·1	25	6·1	24	6·7	22	7·2	24	11·0	23	11·2
		27	5·6	29	9·6	28	10·2	26	10·7	28	14·5	27	14·5
		31	9·1					30	14·2				

TITANIA.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
8	14·9	4	17·8	8	14·1	4	17·5	8	14·1	3	17·3	8	13·0
17	7·9	13	10·9	17	7·2	13	10·7	17	7·2	12	10·3	17	5·8
26	0·8	22	3·9	26	0·4	22	3·8	26	0·3	21	3·3	25	22·6
		30	21·0			30	21·0			29	20·2	November.	
												3	15·3

OBERON.

May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h
8	14·5	4	13·3	1	12·5	10	23·3	6	22·1	3	20·2
22	1·8	18	0·9	15	0·1	24	10·8	20	9·2	17	7·0
				28	11·8						

Apparent Apsides.

	Position-angle.	Apparent Distances.			
		Ariel.	Umbriel.	Titania.	Oberon.
	°	″	″	″	″
April 10	197·0	13·8	19·2	31·5	42·1
June 29	199·5	14·4	20·0	32·8	43·9
Sept. 17	202·6	13·8	19·2	31·5	42·1

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.		February.		March.		April.		September.		October.		November.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
0	18·2	5	0·9	6	10·4	4	19·7	4	12·6	3	21·5	2	6·7	1	16·1
6	15·3	10	22·1	12	7·5	10	16·7	10	9·5	9	18·5	8	3·7	7	13·2
12	12·4	16	19·2	18	4·6	16	13·8	16	6·5	15	15·5	14	0·8	13	10·3
18	9·6	22	16·3	24	1·6	22	10·8	22	3·5	21	12·6	19	21·9	19	7·5
24	6·7	28	13·3	29	22·7			28	0·5	27	9·6	25	19·0	25	4·6
30	3·8													31	1·7

Position-angle and distance of the apse:—Jan. 0, 271°·4, 16″·9; April 10, 269°·7, 16″·2; Sept. 22, 275°·8, 16″·2; Dec. 31, 274°·5, 16″·8.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1906.	P.	D.	L.	1906.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 2 22	−3 11	275 29	July 5	− 1 0	+3 27	353 44
6	− 0 4	3 44	209 39	10	+ 1 17	3 58	287 33
11	2 29	4 17	143 49	15	3 33	4 28	221 24
16	4 53	4 47	77 58	20	5 45	4 55	155 12
21	7 13	5 15	12 8	25	7 55	5 21	89 4
26	9 27	5 41	306 19	30	9 59	5 44	22 56
31	11 36	6 3	240 29	Aug. 4	12 0	6 5	316 48
Feb. 5	13 34	6 23	174 39	9	13 55	6 24	250 43
10	15 33	6 40	108 50	14	15 42	6 40	184 36
15	17 20	6 53	42 58	19	17 24	6 53	118 31
20	18 58	7 3	337 7	24	18 58	7 3	52 28
25	20 28	7 11	271 16	29	20 23	7 10	346 24
Mar. 2	21 48	7 14	205 26	Sept. 3	21 41	7 14	280 22
7	22 59	7 15	139 33	8	22 51	7 15	214 17
12	24 0	7 12	73 38	13	23 52	7 13	148 17
17	24 51	7 5	7 44	18	24 43	7 7	82 17
22	25 32	6 56	301 49	23	25 24	6 57	16 17
27	26 1	6 43	235 54	28	25 56	6 47	310 19
Apr. 1	26 22	6 28	169 56	Oct. 3	26 18	6 33	244 19
6	26 30	6 9	103 56	8	26 29	6 15	178 20
11	26 27	5 49	37 57	13	26 28	5 55	113 23
16	26 13	5 25	331 57	18	26 17	5 32	46 27
21	25 50	4 59	265 55	23	25 54	5 6	340 30
26	25 14	4 31	199 51	28	25 20	4 38	274 34
May 1	24 26	4 2	133 45	Nov. 2	24 33	4 8	208 38
6	23 30	3 30	67 40	7	23 35	3 36	142 43
11	22 22	2 58	1 33	12	22 26	3 2	76 47
16	21 4	2 24	295 26	17	20 2	2 27	10 53
21	19 36	1 49	229 18	22	19 29	1 50	304 57
26	17 58	1 13	163 7	27	17 48	1 13	239 4
31	16 12	0 37	96 58	Dec. 2	15 50	+0 35	173 10
June 5	14 18	−0 1	30 48	7	13 51	−0 4	107 17
10	12 18	+0 35	324 38	12	11 42	0 42	41 24
15	10 12	1 12	258 28	17	9 26	1 20	335 31
20	8 1	1 47	192 15	22	7 5	1 57	269 40
25	5 48	2 21	126 5	27	4 41	2 34	203 49
30	− 3 32	+2 55	59 54	32	+ 2 14	−3 10	137 57

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 26′. In computing L (the heliographic longitude of the centre of the Sun's disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1906·0.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	V Sculptoris	0 3 52	−39 45·1	65.	S Tauri	4 24 3	+ 9 44·3
2.	S Sculptoris	0 10 37	−32 34·1	66.	T Camelopardi	4 30 56	+65 57·5
3.	X Andromedæ	0 11 13	+46 29·4	67.	R Reticuli	4 32 34	−63 13·4
4.	T Ceti	0 17 1	−20 34·8	68.	X Camelopardi	4 33 22	+74 56·7
5.	T Andromedæ	0 17 29	+26 28·4	69.	R Doradûs	4 35 40	−62 15·7
6.	T Cassiopeiæ	0 18 8	+55 16·3	70.	R Cæli	4 37 15	−38 25·1
7.	S Toucani	0 18 41	−62 11·8	71.	R Pictoris	4 43 39	−49 24·9
8.	R Andromedæ	0 19 4	+38 3·4	72.	V Tauri	4 46 35	+17 22·8
9.	S Ceti	0 19 17	− 9 50·9	73.	U Leporis	4 52 16	−21 22·0
10.	T Sculptoris	0 24 35	−38 25·7	74.	R Orionis	4 53 54	+ 7 59·3
11.	Y Cephei	0 31 43	+79 50·4	75.	ε Aurigæ	4 55 13	+43 41·1
12.	U Cassiopeiæ	0 41 6	+47 44·6	76.	R Leporis	4 55 19	−14 56·9
13.	W^{2} Andromedæ	0 42 15	+32 10·4	77.	W Orionis	5 0 32	+ 1 2·9
14.	V Andromedæ	0 44 59	+35 8·5	78.	T Leporis	5 0 50	−22 1·9
15.	X Sculptoris	0 44 59	−35 25·8	79.	V Orionis	5 1 6	+ 3 58·5
16.	R^{2} Andromedæ	0 46 17	+33 51·9	80.	S Pictoris	5 8 27	−48 37·3
17.	W Cassiopeiæ	0 49 20	+58 3·3	81.	R Aurigæ	5 9 42	+53 28·9
18.	U Cephei	0 53 54	+81 22·2	82.	T Columbæ	5 15 51	−33 48·3
19.	Z Ceti	1 1 57	− 1 59·1	83.	W Aurigæ	5 20 33	+36 49·2
20.	32 Cassiopeiæ	1 5 33	+64 31·1	84.	Y Aurigæ	5 21 57	+42 21·5
21.	U Sculptoris	1 7 7	−30 36·9	85.	R^{2} Camelopardi	5 24 7	+72 23·1
22.	U Andromedæ	1 10 7	+40 13·3	86.	S Orionis	5 24 22	− 4 46·0
23.	S Piscium	1 12 39	+ 8 26·2	87.	S Camelopardi	5 30 52	+68 44·9
24.	S Cassiopeiæ	1 12 44	+72 7·0	88.	X Orionis	5 32 54	− 1 49·7
25.	U Piscium	1 18 0	+12 22·5	89.	R^{2} Tauri	5 33 38	+26 19·2
26.	R Sculptoris	1 22 39	−33 1·8	90.	U Aurigæ	5 36 2	+31 59·6
27.	R Piscium	1 25 47	+ 2 23·8	91.	Y Orionis	5 36 54	− 4 11·1
28.	U^{2} Andromedæ	1 33 8	+38 11·3	92.	S Columbæ	5 43 24	−31 43·5
29.	Y Andromedæ	1 34 6	+38 52·0	93.	R Columbæ	5 46 54	−29 13·1
30.	X Cassiopeiæ	1 50 10	+58 47·9	94.	Z Tauri	5 47 1	+15 46·3
31.	U Persei	1 53 20	+54 21·9	95.	U^{2} Tauri	5 47 12	+15 56·6
32.	S Arietis	1 59 35	+12 4·6	96.	V Camelopardi	5 50 11	+74 30·0
33.	V^{2} Andromedæ	2 4 57	+48 29·3	97.	U Orionis	5 50 14	+20 9·5
34.	R Arietis	2 10 46	+24 37·2	98.	Z Aurigæ	5 54 8	+53 18·1
35.	W Andromedæ	2 11 37	+43 52·2	99.	R Octantis	5 54 58	−86 26·0
36.	Z Cephei	2 13 35	+81 14·7	100.	X Aurigæ	6 4 53	+50 15·0
37.	*o* Ceti	2 14 36	− 3 24·3	101.	η Geminorum	6 9 12	+22 32·1
38.	R Ceti	2 21 13	− 0 36·1	102.	W Camelopardi	6 12 49	+75 31·9
39.	R^{2} Persei	2 22 8	+50 51·0	103.	V Aurigæ	6 16 45	+47 42·6
40.	S Horologii	2 22 33	−59 59·6	104.	V Monocerotis	6 17 59	− 2 8·9
41.	R Fornacis	2 25 3	−26 30·9	105.	T Monocerotis	6 20 9	+ 7 8·3
42.	X Eridani	2 27 40	−41 52·5	106.	T^{4} Aurigæ	6 22 32	+30 33·1
43.	U Ceti	2 29 13	−13 33·6	107.	W Geminorum	6 29 34	+15 24·2
44.	R^{2} Cephei	2 30 12	+80 43·9	108.	S Lyncis	6 36 27	+58 0·2
45.	R Trianguli	2 31 20	+33 51·3	109.	T^{2} Geminorum	6 41 4	+18 42·8
46.	Z Persei	2 34 3	+41 47·7	110.	X Geminorum	6 41 6	+30 22·3
47.	T Arietis	2 43 5	+17 7·1	111.	W Monocerotis	6 47 47	− 7 2·0
48.	W Persei	2 43 39	+56 35·6	112.	U^{2} Monocerotis	6 49 41	− 7 28·6
49.	R Horologii	2 50 45	−50 16·4	113.	Y Monocerotis	6 51 39	+11 21·9
50.	T Horologii	2 57 52	−51 0·8	114.	R Lyncis	6 53 33	+55 27·8
51.	Algol	3 2 3	+40 35·7	115.	S^{2} Geminorum	6 55 37	+30 39·4
52.	U Arietis	3 5 50	+14 26·7	116.	ζ Geminorum	6 58 32	+20 42·5
53.	X Ceti	3 14 38	− 1 24·6	117.	R Geminorum	7 1 42	+22 51·0
54.	T^{2} Persei	3 17 9	+46 13·5	118.	V Canis Min.	7 1 53	+ 9 1·0
55.	Y Persei	3 21 19	+43 50·9	119.	R Canis Min.	7 3 33	+10 10·3
56.	R Persei	3 24 4	+35 20·9	120.	R Volantis	7 7 20	−72 51·9
57.	U^{2} Persei	3 24 20	+39 20·2	121.	L^{2} Puppis	7 10 40	−44 29·4
58.	T Fornacis	3 25 39	−28 43·7	122.	R^{2} Monocerotis	7 12 46	+ 1 16·2
59.	U Eridani	3 46 30	−25 14·5	123.	R Canis Maj.	7 15 13	−16 13·0
60.	T Eridani	3 51 12	−24 18·5	124.	R^{2} Geminorum	7 15 34	+31 3·5
61.	λ Tauri	3 55 28	+12 13·6	125.	T Canis Maj.	7 17 33	−25 16·1
62.	*W^{2} Tauri*	3 58 7	+27 52·0	126.	V Geminorum	7 17 54	+13 16·
63.	*W Eridani*	4 7 34	−25 22·5	127.	U^{2} Geminorum	7 21 21	+21 37
64.	*R Tauri*	4 23 9	+ 9 57·2	128.	U Monocerotis	7 26 19	− 9

Mean Places of Variable Stars for 1906·0 (continued).

No.	Star.	R.A.			Dec.		No.	Star.	R.A.			Dec.	
		h	m	s	°	'			h	m	s	°	'
129.	S Canis Min.	7	27	37	+8	31·2	194.	Z Hydræ	11	42	55	−32	44·8
130.	Y Camelopardi	7	28	28	+76	16·1	195.	X Centauri	11	44	30	−41	13·9
131.	Z Puppis	7	28	34	−20	27·7	196.	W Centauri	11	50	20	−58	43·8
132.	X Puppis	7	28	42	−20	42·5	197	R Comæ	11	59	26	+19	18·5
133.	T Canis Min.	7	28	47	+11	56·8	198	S Muscæ	12	7	43	−69	37·7
134.	S Volantis	7	31	23	−73	10·6	199.	T Virginis	12	9	47	−5	30·8
135.	U Canis Min.	7	36	14	+8	36·0	200.	R Corvi	12	14	46	−18	43·9
136.	S Geminorum	7	37	24	+23	40·3	201	T Crucis	12	16	13	−61	45·6
137.	W Puppis	7	42	51	−41	58·0	202.	R Crucis	12	18	28	−61	6·5
138.	T Geminorum	7	43	40	+23	58·1	203.	T Canum Ven.	12	25	33	+32	1·4
139.	R^2 Puppis	7	43	43	−41	8·8	204.	U Centauri	12	28	19	−54	8·5
140.	U Geminorum	7	49	32	+22	15·0	205.	Y Virginis	12	29	2	−3	54·2
141.	V Puppis	7	55	32	−48	59·4	206.	T Ursæ Maj.	12	32	7	+60	0·3
142.	U Puppis	7	56	25	−12	34·8	207.	R Virginis	12	33	44	+7	30·3
143.	S^2 Puppis	8	9	28	−34	17·6	208.	R Muscæ	12	36	20	−68	53·5
144.	Y Puppis	8	9	4	−34	51·4	209	S Ursæ Maj.	12	39	50	+61	36·5
145.	R Cancri	8	11	23	+12	0·9	210.	U^2 Virginis	12	42	31	+4	39·5
146.	Z Camelopardi	8	14	46	+73	24·5	211.	U Virginis	12	46	20	+6	3·9
147.	V Cancri	8	16	22	+17	35·0	212.	S Crucis	12	48	48	−57	55·2
148.	R Chamæleontis	8	23	55	−76	3·0	213.	T^2 Virginis	12	57	52	+5	41·5
149.	V Carinæ	8	26	49	−59	48·5	214.	U Octantis	13	12	53	−83	43·9
150.	X Carinæ	8	29	14	−58	54·4	215.	W Virginis	13	21	11	−2	53·5
151.	U Cancri	8	30	23	+19	13·2	216.	V Virginis	13	22	57	−2	41·2
152.	X Ursæ Maj.	8	34	20	+50	27·8	217.	R Hydræ	13	24	35	−22	47·7
153.	T Velorum	8	34	37	−47	1·9	218.	S Virginis	13	28	5	−6	42·7
154.	S Cancri	8	38	34	+19	22·4	219.	T Ursæ Minoris.	13	32	45	+73	54·5
155.	R Mali	8	41	32	−27	51·5	220.	T Centauri	13	36	22	−33	7·3
156.	S Hydræ	8	48	40	+3	25·4	221.	T^2 Centauri	13	42	51	−36	23·6
157.	T Hydræ	8	51	6	−8	46·9	222.	W Hydræ	13	43	44	−27	53·8
158.	T Cancri	8	51	18	+20	12·6	223	R Canum Ven.	13	44	55	+40	0·6
159	S Mali	9	0	56	−24	42·8	224.	X^2 Centauri	13	45	54	−36	28·6
160.	V Ursæ Maj.	9	1	38	+51	29·5	225.	T Apodis	13	46	41	−77	20·3
161.	W Cancri	9	4	24	+25	37·9	226	R^2 Virginis	13	59	54	−8	44·9
162.	W' Carinæ	9	18	17	−68	21·5	227.	Z Boötis	14	1	57	+13	56·8
163.	V Velorum	9	19	26	−55	33·5	228.	Z Virginis	14	5	17	−12	51·5
164.	Y Velorum	9	25	52	−51	46·2	229.	U^2 Hydræ	14	6	9	−28	26·5
165.	S Antliæ	9	28	12	−28	12·8	230.	R Centauri	14	9	48	−59	28·5
166.	S Velorum	9	29	41	−44	47·5	231.	R^2 Centauri	14	10	20	−57	24·9
167.	U Velorum	9	29	41	−45	5·9	232.	U Ursæ Minoris	14	15	2	+67	8·3
168.	R Carinæ	9	29	53	−62	22·4	233.	Y Boötis	14	17	39	+20	14·1
169.	X Hydræ	9	31	1	−14	16·3	234.	S Boötis	14	19	44	+54	14·3
170.	Y Draconis	9	31	47	+78	16·6	235.	S^2 Virginis	14	22	34	+5	6·0
171.	W Ursæ Maj.	9	37	9	+56	23·0	236.	R Camelopardi	14	24	38	+84	15·5
172.	R Leo. Min.	9	39	57	+34	56·8	237.	Y Centauri	14	25	27	−29	40·7
173.	R^2 Hydræ	9	40	41	−23	35·2	238.	V Centauri	14	25	49	−56	28·3
174.	R Leonis	9	42	30	+11	51·9	239.	V Boötis	14	25	58	+39	16·8
175.	*l* Carinæ	9	42	40	−62	4·5	240.	R Boötis	14	33	3	+27	8·6
176.	Z Velorum	9	49	38	−53	44·2	241.	V Libræ	14	35	8	−17	15·2
177.	V Leonis	9	54	49	+21	42·7	242.	S Lupi	14	47	8	−46	13·7
178.	R^2 Carinæ	9	55	2	−58	24·7	243.	U Boötis	14	49	59	+18	4·6
179.	V^2 Carinæ	9	55	43	−63	26·8	244.	δ Libræ	14	55	57	−8	8·7
180.	S Carinæ	10	6	23	−61	5·4	245.	S Apodis	14	59	57	−71	41·8
181.	Z Carinæ	10	10	39	−58	23·3	246.	T^2 Libræ	15	1	7	−18	22·2
182.	W Velorum	10	11	44	−54	0·7	247.	T Libræ	15	5	22	−19	39·6
183.	R^2 Velorum	10	18	3	−41	52·9	248.	Y Libræ	15	6	43	−5	39·4
184.	Y Carinæ	10	29	38	−58	0·8	249.	R Triang. Austr.	15	11	21	−66	9·1
185.	Z^2 Carinæ	10	32	55	−70	13·4	250.	U Coronæ	15	14	22	+31	59·4
186.	R Ursæ Maj.	10	38	1	+69	16·2	251.	S Libræ	15	15	59	−20	2·9
187.	S^2 Hydræ	10	46	50	−28	8·0	252.	S Serpentis	15	17	15	+14	39·0
188.	V Hydræ	10	47	3	−20	45·0	253.	S Coronæ	15	17	34	+31	42·3
189.	W Leonis	10	48	40	+14	13·0	254	S^2 Libræ	15	18	50	−22	34·5
190.	U Carinæ	10	53	58	−59	13·8	255.	U^2 Libræ	15	28	1	−15	0·6
191.	*S Leonis*	*11*	5	59	+5	58·3	256.	R Normæ	15	29	11	−49	11·6
192.	*S^2 Centauri*	*11*	16	22	−61	21·4	257.	X Libræ	15	30	47	−20	51·2
193.	*Z Draconis*	*11*	40	9	+72	47·0	258.	W Libræ	15	32	32	−15	51·8

Mean Places of Variable Stars for 1906·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
259.	S Ursæ Minoris.	15 33 12	+78 57·1	324.	W Sagittarii ...	17 59 2	−29 35·2
260.	U Normæ	15 35 5	−55 0·5	325.	R Pavonis	18 3 51	−63 38·1
261.	U Libræ	15 36 34	−20 52·7	326.	T Herculis	18 5 33	+31 0·2
262.	T Normæ	15 36 49	−54 41·2	327.	S^2 Sagittarii ...	18 11 22	−34 8·4
263.	Z Libræ	15 41 2	−20 49·7	328.	V Serpentis......	18 11 25	−15 33·3
264.	V Coronæ	15 46 10	+39 51·2	329.	W Lyræ	18 11 40	+36 38·4
265.	R Serpentis......	15 46 22	+15 25·1	330.	Y Sagittarii......	18 15 51	−18 54·2
266.	R Lupi	15 47 22	−36 1·1	331.	V^2 Sagittarii ...	18 21 45	−33 22·7
267.	R Libræ	15 48 17	−15 57·4	332.	*d* Serpentis	18 22 24	+ 0 8·4
268.	R^2 Libræ.........	15 50 59	−18 1·7	333.	U^3 Herculis ...	18 22 32	+24 58·2
269.	S Triang. Austr.	15 52 44	−63 30·5	334.	T Serpentis......	18 24 13	+ 6 14·0
270.	U Lupi	15 54 52	−29 39·3	335.	X^2 Herculis......	18 26 17	+12 32·8
271.	U Triang. Austr.	15 58 57	−62 39·3	336.	U Sagittarii ...	18 26 21	−19 11·7
272.	Z^2 Scorpii	15 58 59	−23 50·5	337.	Z^2 Herculis	18 32 59	+25 58·2
273.	X Herculis	15 59 50	+47 29·7	338.	X Ophiuchi ...	18 33 52	+ 8 44·7
274.	Z Scorpii.........	16 0 29	−21 28·6	339.	Y Lyræ	18 34 24	+43 52·2
275.	R^2 Herculis......	16 1 38	+50 45·3	340.	U Coron. Austr.	18 34 42	−37 55·3
276.	R Herculis	16 1 59	+18 37·4	341.	Z Coron. Austr.	18 34 51	−37 34·2
277.	U Serpentis ...	16 2 47	+10 11·1	342.	S^2 Draconis......	18 40 5	+74 14·4
278.	X Scorpii	16 3 1	−21 16·4	343.	Z^2 Lyræ	18 40 8	+32 42·0
279.	W Scorpii	16 6 16	−19 53·6	344.	R^2 Draconis ...	18 40 52	+62 34·9
280.	U^2 Herculis ...	16 6 18	+25 19·0	345.	Y^2 Lyræ	18 41 28	+34 34·4
281.	S Normæ	16 11 4	−57 40·1	346.	W^2 Lyræ.........	18 42 18	+43 32·3
282.	R Scorpii	16 12 3	−22 42·8	347.	R Scuti	18 42 28	− 5 48·4
283.	W Coronæ	16 12 3	+38 1·8	348.	V Scuti	18 42 52	−12 14·0
284.	S Scorpii.........	16 12 4	−22 39·7	349.	β Lyræ	18 46 37	+33 15·2
285.	W Ophiuchi ..	16 16 21	− 7 28·5	350.	κ Pavonis	18 47 16	−67 21·1
286.	V Ophiuchi ...	16 21 30	−12 12·9	351.	U Scuti	18 49 11	−12 43·4
287.	U Herculis	16 21 38	+19 6·4	352.	X^2 Lyræ	18 50 40	+32 42·7
288.	Y Scorpii	16 24 10	−19 14·1	353.	R Lyræ	18 52 28	+43 49·3
289.	T Ophiuchi......	16 28 21	−15 55·9	354.	R Coron. Austr.	18 55 33	−37 5·2
290.	R^3 Herculis ...	16 28 21	+ 7 3·4	355.	Z Lyræ	18 56 13	+34 49·5
291.	S Ophiuchi......	16 28 51	−16 57·8	356.	T^2 Lyræ	18 57 59	+37 23·2
292.	R Aræ............	16 31 56	−56 48·4	357.	R Aquilæ	19 1 51	+ 8 5·3
293.	W Herculis......	16 31 53	+37 31·9	358.	V Lyræ	19 5 24	+29 30·4
294.	R Draconis ...	16 32 25	+66 57·0	359.	X^2 Sagittarii ...	19 9 3	−18 58·2
295.	T^3 Scorpii	16 34 36	−32 11·8	360.	U^2 Lyræ.........	19 9 17	+41 8·7
296.	S Draconis	16 40 56	+55 5·8	361.	S Lyræ	19 9 21	+25 50·6
297.	R^2 Ophiuchi ...	16 43 32	−19 18·0	362.	S^2 Lyræ	19 9 31	+33 15·2
298.	S Herculis	16 47 37	+15 6·0	363.	U Draconis......	19 9 57	+67 7·5
299.	S^2 Scorpii	16 48 48	−44 57·0	364.	W Aquilæ	19 10 19	− 7 12·4
300.	R^2 Scorpii	16 50 38	−30 25·8	365.	T Sagittarii ...	19 10 49	−17 8·1
301.	V^2 Scorpii	16 52 11	−33 27·8	366.	R Sagittarii ...	19 11 11	−19 28·4
302.	V^2 Herculis......	16 56 58	+31 21·8	367.	V^2 Lyræ	19 12 44	+32 15·4
303.	R Ophiuchi......	17 2 22	−15 58·1	368.	S Sagittarii......	19 13 56	−19 11·7
304.	T^2 Herculis......	17 7 1	+27 10·3	369.	Z Sagittarii......	19 14 9	−21 6·0
305.	W^2 Scorpii	17 8 42	−33 19·5	370.	U Sagittæ	19 14 41	+19 26·3
306.	U Ophiuchi......	17 11 45	+ 1 18·9	371.	U Lyræ	19 16 50	+37 42·2
307.	Z Ophiuchi......	17 14 46	+ 1 36·9	372.	T Sagittæ	19 17 30	+17 28·5
308.	S^2 Herculis......	17 17 46	+23 0·7	373.	R^2 Lyræ	19 22 29	+42 36·2
309.	S Octantis	17 28 32	−86 46·3	374.	U Aquilæ	19 24 18	− 7 14·2
310.	V^2 Ophiuchi ...	17 30 2	+ 7 18·6	375.	V^4 Cygni.........	19 30 5	+28 7·0
311.	U^2 Scorpii	17 35 38	−43 42·2	376.	U Vulpeculæ ...	19 32 31	+20 7·4
312.	X Sagittarii ...	17 41 38	−27 47·7	377.	T^2 Aquilæ	19 33 36	+11 30·5
313.	U^3 Scorpii	17 42 0	−35 40·1	378.	R Cygni	19 34 18	+49 59·4
314.	Y^2 Scorpii	17 44 40	−33 40·7	379.	V^2 Aquilæ	19 36 14	+ 9 42·3
315.	T^3 Herculis......	17 45 5	+22 34·2	380.	T Pavonis	19 40 11	−72 0·3
316.	Y Ophiuchi......	17 47 36	− 6 7·3	381.	T^2 Cygni.........	19 41 0	+48 32·7
317.	W^2 Ophiuchi ...	17 50 49	+ 7 50·5	382.	T^3 Cygni.........	19 41 3	+29 2·2
318.	S Aræ	17 51 56	−49 25·3	383.	X^3 Cygni.........	19 42 57	+32 28·4
319.	T^2 Ophiuchi ..	17 52 8	+11 11·0	384.	R^1 Cygni.........	19 43 31	+48 50·6
320.	Z Herculis	17 53 52	+15 8·8	385.	S Vulpeculæ ...	19 44 32	+27 3·1
321.	T *Draconis*	*17 54 58*	+58 13·6	386.	X Aquilæ.........	19 46 49	+ 4 13·4
322.	*Y^2 Herculis......*	*17 55 40*	+19 29·3	387.	χ Cygni	19 46 57	+32 40
323.	*V Draconis......*	*17 56 24*	+54 52·7	388.	S Pavonis	19 47 18	−59 2

Mean Places of Variable Stars for 1906·0 (continued).

No.	Star.	R.A.			Dec.	
		h	m	s	°	′
389.	η Aquilæ.........	19	47	41	+ 0	45·8
390.	R^3 Sagittarii ...	19	50	6	−29	26·1
391.	S Sagittæ	19	51	44	+16	23·1
392.	U^2 Sagittarii ...	19	52	15	−42	6·0
393.	R^2 Aquilæ	19	52	43	− 2	8·2
394.	X Vulpeculæ ...	19	53	34	+26	18·2
395.	S^2 Aquilæ	19	54	2	− 8	8·3
396.	Z Cygni	19	58	48	+49	46·8
397.	Z^5 Cygni	20	0	48	+41	19·2
398.	S Cygni	20	3	32	+57	42·9
399.	V^3 Cygni	20	4	1	+46	1·6
400.	R Capricorni ...	20	6	2	−14	33·0
401.	S Aquilæ.........	20	7	18	+15	20·4
402.	W^2 Aquilæ	20	7	33	+15	46·7
403.	R Telescopii ...	20	8	8	−47	16·9
404.	U^2 Aquilæ	20	8	20	+12	42·8
405.	Z^2 Sagittarii ...	20	8	55	−44	41·5
406.	W Capricorni...	20	8	57	−22	15·9
407.	R Sagittæ	20	9	46	+16	26·5
408.	S^2 Cygni	20	9	58	+38	26·6
409.	Z Aquilæ.........	20	10	11	− 6	26·3
410.	R Delphini	20	10	23	+ 8	48·2
411.	T^2 Sagittarii ...	20	11	29	−39	24·2
412.	V^5 Cygni	20	11	35	+34	12·9
413.	W^3 Cygni	20	11	47	+30	47·1
414.	V Sagittæ	20	16	2	+20	48·4
415.	U Cygni	20	16	41	+47	35·8
416.	Z^4 Cygni	20	19	49	+42	56·1
417.	U Microscopii...	20	22	59	−40	43·7
418.	W^2 Cygni	20	25	26	+39	40·0
419.	U^2 Capricorni...	20	27	3	−22	0·4
420.	Z Delphini	20	28	21	+17	7·9
421.	Y^3 Cygni	20	29	50	+46	16·8
422.	S^3 Cygni	20	30	5	+54	38·8
423.	V Vulpeculæ ...	20	32	32	+26	16·6
424.	W Delphini	20	33	23	+17	57·3
425.	R Microscopii...	20	34	20	−29	7·4
426.	S Capricorni ...	20	36	14	−19	43·5
427.	Y Delphini	20	37	10	+11	32·5
428.	V Cygni	20	38	16	+47	48·3
429.	S Delphini	20	38	45	+16	45·0
430.	Y Aquarii	20	39	28	− 5	10·7
431.	X Cygni	20	39	43	+35	14·8
432.	T Delphini	20	41	0	+16	3·4
433.	W Aquarii	20	41	29	− 4	25·6
434.	V Aquarii	20	42	4	+ 2	5·6
435.	R^2 Cygni.........	20	42	49	+44	31·0
436.	U Capricorni ...	20	42	55	−15	7·8
437.	V Delphini	20	43	30	+18	59·3
438.	T Aquarii	20	44	59	− 5	29·7
439.	T Vulpeculæ ...	20	47	29	+27	53·6
440.	U Pavonis	20	47	42	−63	3·9
441.	Y Cygni	20	48	19	+34	18·3
442.	Z^2 Cygni	20	48	44	+47	0·1
443.	S Indi	20	49	26	−54	41·0
444.	X Delphini	20	50	34	+17	17·0
445.	R^5 Cygni.........	20	51	10	+30	3·3
446.	S^5 Cygni	20	52	31	+30	4·2
447.	W^5 Cygni	20	53	48	+39	48·9
448.	U^4 Cygni	20	56	38	+42	13·8
449.	R^2 Capricorni..	20	56	45	−27	27·6
450.	*T Octantis ......*	20	58	26	−82	28·6
451.	*R Vulpeculæ ...*	*21*	*0*	*12*	+23	26·9
452.	*X^5 Cygni.........*	*21*	*0*	*40*	+39	35·7
453.	*T^4 Cygni ....*	*21*	*2*	*0*	+29	1·7
454.	V Capricorni ...	21	2	9	−24	18·2
455.	U^5 Cygni.........	21	2	33	+45	24·0
456.	X Capricorni ...	21	3	10	−21	43·6
457.	X Cephei	21	3	14	+82	41·3
458.	Z Capricorni ...	21	5	23	−16	33·8
459.	S^2 Aquarii	21	6	3	− 4	24·6
460.	T Cephei.........	21	8	18	+68	6·5
461.	R Equulei	21	8	41	+12	24·9
462.	R^2 Aquarii	21	10	8	− 3	17·1
463.	X Pegasi	21	16	32	+14	3·1
464.	T Capricorni ...	21	16	50	−15	33·4
465.	T^6 Cygni	21	18	53	+41	59·6
466.	S Microscopii ...	21	21	9	−30	15·4
467.	Y Capricorni ...	21	29	15	−14	23·5
468.	W Cygni.........	21	32	30	+44	57·3
469.	S Cephei	21	36	24	+78	12·1
470.	U^2 Cygni.........	21	37	28	+53	53·9
471.	R^3 Cygni	21	39	1	+43	9·3
472.	V^2 Cygni	21	39	23	+37	35·2
473.	R^2 Pegasi	21	40	15	+24	34·5
474.	R Gruis	21	42	28	−47	20·9
475.	S^6 Cygni	21	45	0	+43	48·7
476.	Y^6 Cygni.........	21	47	56	+42	41·6
477.	T^5 Cygni	21	55	28	+43	53·5
478.	V Pegasi	21	56	20	+ 5	40·1
479.	U Aquarii	21	58	12	−17	4·8
480.	S Piscis Austr. .	21	58	23	−28	30·3
481.	T^2 Pegasi	22	0	4	+34	40·0
482.	W Lacertæ	22	3	27	+37	16·7
483.	T Pegasi	22	4	18	+12	4·8
484.	Y Pegasi	22	7	4	+13	54·1
485.	S^2 Pegasi.........	22	7	42	+14	5·3
486.	R Piscis Austr. .	22	12	40	−30	4·4
487.	X Aquarii	22	13	29	−21	22·2
488.	T Gruis	22	20	13	−38	2·6
489.	S Gruis	22	20	18	−48	54·6
490.	S Lacertæ	22	24	53	+39	50·1
491.	δ Cephei	22	25	41	+57	56·0
492.	R Indi	22	29	19	−67	46·4
493.	W Cephei	22	32	53	+57	56·5
494,	T Toucani	22	34	22	−62	2·6
495.	R Lacertæ	22	39	6	+41	52·7
496.	U Lacertæ	22	43	51	+54	39·7
497.	V Lacertæ	22	44	48	+55	49·5
498.	S Aquarii	22	52	4	−20	50·7
499.	R Pegasi	23	1	56	+10	2·2
500	Z^2 Andromedæ .	23	7	17	+52	23·0
501.	V Cassiopeiæ ...	23	7	38	+59	10·4
502.	W Pegasi	23	15	4	+25	45·5
503.	S Pegasi	23	15	47	+ 8	24·3
504.	R Aquarii	23	38	57	−15	48·3
505.	T^2 Cassiopeiæ...	23	41	47	+53	58 0
506.	Z Aquarii	23	47	23	−16	22·7
507.	S^3 Andromedæ .	23	50	37	+48	6·9
508.	R^2 Cassiopeiæ...	23	51	2	+53	11·7
509.	R Phœnicis......	23	51	35	−50	18·6
510.	V Cephei.........	23	52	0	+82	40·1
511.	R Toucani	23	52	31	−65	54·4
512.	V Ceti	23	53	6	− 9	29·0
513.	U Pegasi	23	53	11	+15	25·9
514.	R Cassiopeiæ ...	23	53	37	+50	51·9
515.	S Phœnicis	23	54	13	−57	5·9
516.	Z Pegasi	23	55	17	+25	22 6
517.	W Ceti	23	57	18	−15	11·9
518	Y Cassiopeiæ ...	23	58	28	+55	9·3

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYP

(The No. after the Star's name refers to Catalogue on pp. 23–26.)

Note.—The times of minima only are given; the times of maxima may be found by adding the printed under the name of the star.

T Monocer. 105.

($7^d\ 22\frac{1}{2}^h$.)

Jan.	May	Oct.
d h	d h	d h
5 15	20 17	2 18
		29 18
Feb.	July	
1 16	13 17	
28 16		
Mar.	Aug.	Nov.
27 16	9 18	25 19
Apr.	Sept.	Dec.
23 16	5 18	22 19

W Geminor. 107*

(Min. $7^d\ 18^h$.
Max. $2^d\ 15\frac{1}{2}^h$.)

Jan.	Apr.	Oct.
d h	d h	d h
1 16	20 1	7 8
17 4		22 19
Feb.	May	Nov.
1 15	5 13	7 7
17 3	21 0	22 19
Mar.	Aug.	Dec.
4 14	21 21	8 6
20 2		23 18
	Sept.	
Apr.	6 9	
4 14	21 20	

ζ Geminor. 116.

($5^d\ 0\frac{1}{2}^h$.)

Jan.	Mar.	May
d h	d h	d h
4 18	6 17	6 15
14 22	16 20	16 18
25 2	27 0	26 22
Feb.	Apr.	Aug.
4 6	6 4	6 0
14 9	16 7	16 4
24 13	26 11	26 7

ζ Geminor. 116 (*continued*).

Sept.	Oct.	Nov.
d h	d h	d h
5 11	16 2	25 17
15 15	26 6	
25 18		Dec.
	Nov.	5 20
Oct.	5 9	16 0
5 22	15 13	26 4

X Puppis 132.

($5^d\ 4\frac{1}{2}^h$.)

Jan.	May	Sept.
d d	d h	d h
7 11	17 4	23 22
Feb.	June	Oct.
2 9	12 3	19 21
28 8		
	July	Nov.
Mar.	8 2	14 20
26 7		
	Aug.	Dec.
Apr.	3 1	10 18
21 6	28 23	

V Carinæ 149*.

(Min. $6^d\ 16\frac{1}{2}^h$.
Max. $2^d\ 4^h$.)

Jan.	May	Sept.
d h	d h	d h
3 10	3 22	1 11
16 20	17 8	14 20
30 5	30 17	28 5
Feb.	June	Oct.
12 14	13 3	11 15
26 0	26 12	25 0
Mar.	July	Nov.
11 9	9 21	7 10
24 18	23 7	20 19
		Dec.
Apr.	Aug.	4 4
7 4	5 16	17 14
20 13	19 1	30 23

T Velorum 153*.

(Min. $4^d\ 15^h.3$.
Max. $1^d\ 9\frac{1}{2}^h$.)

Jan.	May	Sept.
d h	d h	d h
0 14	1 5	8 3
9 21	10 12	17 9
19 3	19 18	26 16
28 10	29 1	
Feb.	June	Oct.
6 17	7 8	5 23
16 0	16 14	15 5
25 6	25 21	24 12
Mar.	July	Nov.
6 13	5 4	2 19
15 20	14 10	12 1
25 2	23 17	21 8
		30 15
Apr.	Aug.	Dec.
3 9	2 0	9 21
12 16	11 6	19 4
21 22	20 13	28 11
	29 20	

V Velorum 163*.

(Min. $4^d\ 8^h.9$.
Max. $0^d\ 23\frac{1}{2}^h$.)

Jan.	May	Sept.
d h	d h	d h
0 0	2 9	1 18
8 18	11 3	10 12
17 12	19 21	19 6
26 5	28 15	28 0
Feb.	June	Oct.
3 23	6 8	6 18
12 17	15 2	15 11
21 11	23 20	24 5
Mar.	July	Nov.
2 5	2 14	1 23
10 22	11 8	10 17
19 16	20 1	19 11
28 10	28 19	28 4
Apr.	Aug.	Dec.
6 4	6 13	6 22
14 22	15 7	15 16
23 15	24 1	24 10

Y Carinæ 184*.

(Min. $3^d\ 15^h.4$.
Max. $1^d\ 1\frac{1}{2}^h$.)

Jan.	May	Sept.
d h	d h	d h
1 4	4 23	5 17
8 11	12 5	13 0
15 18	19 12	20 6
23 0	26 19	27 13
30 7		
Feb.	June	Oct.
6 14	3 1	4 2
13 21	10 8	12 3
21 3	17 15	19 9
28 10	24 22	26 16
Mar.	July	Nov.
7 17	2 4	2 23
14 23	9 11	10 5
22 6	16 18	17 12
29 13	24 1	24 19
	31 7	
Apr.	Aug.	Dec.
5 20	7 14	2 2
13 2	14 21	9 8
20 9	22 3	16 15
27 16	29 10	23 22
		31 4

S Muscæ 198*.

(Min. $9^d\ 15\frac{1}{2}^h$.
Max. $3^d\ 11^h$.)

Jan.	May	Sept.
d h	d h	d h
7 9	12 22	5 19
26 17		25 3
Feb.	June	Oct.
5 8	1 6	14 10
24 16	20 13	
Mar.	July	Nov.
15 23	9 21	2 18
	29 4	22 1
Apr.	Aug.	Dec.
4 7	17 12	11 9
23 14		30 16

T Cruci

(Min. 6^d
Max. 2^d

Jan.	Ma
d h	d
3 17	4 2
17 4	18
30 16	31 2
Feb.	Jun
13 3	14
26 14	27 1
Mar.	Jul
12 1	11
25 12	24 1
Apr.	Aug
7 23	7
21 10	20 1

R Cruci

(Min. 5^d
Max. 1^d

Jan.	Ma
d h	d
5 15	2
17 7	13 1
28 23	25 1
Feb.	Jun
9 14	6
21 6	17 1
	29
Mar.	Jul
4 21	11
16 13	22 1
28 5	
	Aug
Apr.	3
8 20	15
20 12	26 1

S Crucis

(Min. 4^d
Max. 1^d

Jan.	Feb
d h	d
0 10	6 2
9 19	16
19 4	25 1
28 14	

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* Alternate minima only are given: the others can be readily found by adding the interval u the name of the *star.*

Variables of Short Period (*continued*).

S Crucis 212 (*continued*).

pr.		July		Oct.	
	h	d	h	d	h
	5	7	1	8	20
	14	16	10	18	5
	0	25	19	27	14
ay		Aug.		Nov.	
	9	4	4	5	23
	18	13	13	15	8
	3	22	22	24	17
	12	Sept.			
ine		1	7	Dec.	
	21	10	16	4	2
	6	20	1	13	11
	15	29	11	22	21

Virginis 215.

($8^{d}\ 4\frac{1}{2}^{h}$.)

an.		Apr.		July	
	h	d	h	d	h
	14	28	5	23	13
	20	May		Aug.	
eb.		15	11	9	20
	3			27	2
ar.		June		Nov.	
	9	1	18	21	11
	16	19	0	Dec.	
pr.		July		8	17
	22	6	7	26	0

Centauri 238*.

(Min. $5^{d}\ 11^{h}{\cdot}9$.
Max. $1^{d}\ 11\frac{1}{2}^{h}$.)

an.		May		Sept.	
	h	d	h	d	h
	15	6	12	4	9
	15	17	12	15	9
	15	28	12	26	8
eb.		June		Oct.	
	14	8	11	7	8
	14	19	11	18	8
		30	11	29	7
ar.		July		Nov.	
	14	11	10	9	7
	14	22	10	20	7
	13				
pr.		Aug.		Dec.	
	13	2	10	1	7
	13	13	10	12	6
	12	24	9	23	6

R Triang. Austr. 249*.

(Min. $3^{d}\ 9^{h}{\cdot}3$.
Max. $1^{d}\ 0\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
3	4	5	4	4	4
9	22	11	23	10	23
16	17	18	17	17	18
23	12	25	12	24	12
30	7	June		Oct.	
Feb.		1	7	1	7
6	1	8	2	8	2
12	20	14	20	14	20
19	15	21	15	21	15
26	9	28	10	28	10
Mar.		July		Nov.	
5	4	5	4	4	5
11	23	11	23	10	23
18	17	18	18	17	18
25	12	25	12	24	13
Apr.		Aug.		Dec.	
1	7	1	7	1	7
8	1	8	2	8	2
14	20	14	20	14	21
21	15	21	18	21	15
28	9	28	10	28	10

U Normæ 260.

($6^{d}\ 0^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	15	8	4	11	17
14	7	20	20	24	9
26	22				
Feb.		June		Oct.	
8	14	2	12	7	1
21	6	15	3	9	16
		27	19		
Mar.		July		Nov.	
5	22	10	11	1	8
18	13	23	2	14	0
31	5			26	16
Apr.		Aug.		Dec.	
12	21	4	18	9	7
25	12	17	10	21	23
		30	2		

S Triang. Austr. 269*.

(Min. $6^{d}\ 7^{h}{\cdot}8$.
Max. $2^{d}\ 2\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
4	22	11	9	2	5
17	14	24	1	14	21
30	5	June		27	12
Feb.		5	16	Oct.	
11	21	18	8	10	4
24	12	30	23	22	19
Mar.		July		Nov.	
9	4	13	15	4	11
21	19	26	7	17	2
Apr.				29	18
3	11	Aug.		Dec.	
16	2	7	22	12	9
28	18	20	14	25	1

S Normæ 281*.

(Min. $9^{d}\ 18^{h}$.
Max. $4^{d}\ 9\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
8	21	5	22	19	10
28	9	25	10		
				Oct.	
Feb.		June		8	23
16	21	13	22	28	11
Mar.		July		Nov.	
8	9	3	10	16	23
27	21	22	22		
Apr.		Aug.		Dec.	
16	19	11	10	6	11
		30	22	25	23

V^{2} Scorpii 301 *.

(Min. $6^{d}\ 1^{h}{\cdot}5$.
Max. $1^{d}\ 10^{h}$.)

Jan.		Mar.		May	
d	h	d	h	d	h
2	14	4	5	3	20
14	17	16	8	15	23
26	20	28	11	28	2
Feb.		Apr.		June	
7	23	9	14	9	5
20	2	21	17	21	8

V^{2} Scorpii 301 (*continued*).

July		Aug.		Oct.	
d	h	d	h	d	h
3	11	20	23	8	11
15	14			20	14
27	17				
		Sept.			
		2	2	Nov.	
Aug.		14	5	1	17
8	20	26	8		

X Sagittar. 312*

(Min. $7^{d}\ 0^{h}{\cdot}3$.
Max. $2^{d}\ 21^{h}$.)

Feb.		May		Sept.	
d	h	d	h	d	h
5	7	28	12	3	16
19	8			17	17
		June			
Mar.		11	13	Oct.	
5	9	25	13	1	17
19	9			15	18
		July		29	18
Apr.		9	14		
2	10	23	14	Nov.	
16	10			12	19
30	11	Aug.		26	19
May		6	15		
14	11	20	15		

Y Ophiuchi 316.

($6^{d}\ 5\frac{1}{2}^{h}$.)

Jan.		May		Aug.	
d	h	d	h	d	h
20	18	3	12	31	8
		21	15		
Feb.		June		Sept.	
6	21	6	18	17	11
24	0	23	20		
				Oct.	
Mar.		July		4	14
13	3	10	23	21	17
30	6	28	2		
				Nov.	
Apr.		Aug.		7	20
16	9	14	5	24	23

W Sagittar.

(Min. $7^{d}\ 14^{h}$
Max. $3^{d}\ 0^{h}$

Feb.		May	
d	h	d	h
4	8	21	15
19	12		
Mar.		June	
6	17	5	20
21	21	21	1
Apr.		July	
6	2	6	5
21	6	21	10
May		Aug.	
6	11	5	14

Y Sagittar. 3

(Min. $5^{d}\ 18^{h}$
Max. $2^{d}\ 2\frac{1}{2}^{h}$

Feb.		May	
d	h	d	h
3	22	18	20
15	11	30	9
27	0		
Mar.		June	
10	13	10	22
22	3	22	12
Apr.		July	
2	16	4	1
14	5	15	14
25	18	27	3
May		Aug.	
7	7	7	16

U Sagittar.

(Min. $6^{d}\ 17$
Max. $2^{d}\ 23$

Feb.		Mar.	
d	h	d	h
4	3	3	2
17	15	16	14

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* *Alternate minima only are given:* the others can be readily found by adding the interval un
the name of the star.

VARIABLES OF SHORT PERIOD (*continued*).

U SAGITTAR. 336 (*continued*).

Apr.		June		Sept.	
d	h	d	h	d	h
12	14	19	0	7	23
26	1			21	11
May		July		Oct.	
9	13	2	12	4	22
23	1	16	0	18	10
		29	12	31	22
June		Aug.		Nov.	
5	13	11	23	14	10
		25	11		

β LYRÆ 349.

(Max. $3^d\ 2^h$.
Secondary Min.
$6^d\ 11^h$.
Max. $9^d\ 18^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	8	10	12	3	19
14	6	23	10	16	17
27	4			29	15
Feb.		June		Oct.	
9	2	5	9	12	13
22	0	18	7	25	11
Mar.		July		Nov.	
6	22	1	5	7	9
19	20	14	3	20	7
		27	1		
Apr.		Aug.		Dec.	
1	18	8	23	3	5
14	16	21	21	16	3
27	14			29	1

κ PAVONIS 350*.

(Min. $9^d\ 2\frac{1}{2}^h$.
Max. $4^d\ 9^h$.)

Jan.		Mar.		May	
d	h	d	h	d	h
5	9	19	4	12	18
23	14			30	23
Feb.		Apr.		June	
10	18	6	9	18	3
28	23	24	13		

κ PAVONIS 350 (*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
6	8	17	3	10	18
24	13			28	22
Aug.		Oct.		Dec.	
11	18	5	8	17	3
29	22	23	13		

U AQUILÆ 374 *.

(Min. $7^d\ 0^h{\cdot}6$.
Max. $2^d\ 4\frac{1}{2}^h$.)

Feb.		May		Aug.	
d	h	d	h	d	h
6	16	30	1	22	8
20	17				
Mar.		June		Sept.	
6	18	13	2	5	9
20	20	27	4	19	10
Apr.		July		Oct.	
3	21	11	5	3	12
17	22	25	6	17	13
				31	14
May		Aug.		Nov.	
1	23	8	7	14	15
16	0			28	16

U VULPEC. 376 *.

(Min. $7^d\ 23\frac{1}{2}^h$.
Max. $2^d\ 3^h$.)

Jan.		Apr.		July	
d	h	d	h	d	h
4	3	9	21	14	15
20	2	25	20	30	14
Feb.		May		Aug.	
5	1	11	19	15	13
21	0	27	18	31	12
Mar.		June		Sept.	
8	23	12	17	16	11
24	22	28	16		

U VULPEC. 376 (*continued*).

Oct.		Nov.		Dec.	
d	h	d	h	d	h
2	10	3	9	5	7
18	9	19	8	21	6

T^3 CYGNI 382 *.

(Min. $3^d\ 20^h{\cdot}3$.
Max. $1^d\ 8^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	6	5	8	5	9
9	23	13	0	13	2
17	15	20	17	20	19
25	8	28	10	28	11
Feb.		June		Oct.	
2	0	5	2	6	4
9	17	12	19	13	21
17	10	20	11	21	13
25	2	28	4	29	6
Mar.		July		Nov.	
4	19	5	21	5	22
12	12	13	13	13	15
20	4	21	6	21	8
27	21	28	22	29	0
Apr.		Aug.		Dec.	
4	13	5	15	6	17
12	6	13	8	14	9
19	23	21	0	22	2
27	15	28	17	29	19

η AQUILÆ 389 *.

(Min. $7^d\ 4^h$.
Max. $2^d\ 9^h$.)

Feb.		June		Sept.	
d	h	d	h	d	h
9	5	4	1	26	21
23	13	18	9		
Mar.		July		Oct.	
9	22	2	18	11	5
24	6	17	2	25	13
		31	11		
Apr.		Aug.		Nov.	
7	15	14	19	8	22
21	23	29	4	23	6
May		Sept.		Dec.	
6	8	12	12	7	15
20	17			21	23

S SAGITTÆ 391*.

(Min. $8^d\ 9^h$.
Max. $2^d\ 23^h$.)

Feb.		May		Sept.	
d	h	d	h	d	h
3	12	31	20	9	10
20	6			26	5
Mar.		June		Oct.	
9	1	17	15	12	23
25	19			29	17
Apr.		July		Nov.	
11	13	4	9	15	12
28	8	21	3		
May		Aug.		Dec.	
15	2	6	22	2	6
		23	16	19	0

X CYGNI 431.

($6^d\ 5^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
3	5	14	7	5	23
19	14	30	16	22	9
Feb.		June		Oct.	
4	23	16	1	8	18
21	9			25	3
Mar.		July		Nov.	
9	18	2	10	10	12
26	3	18	20	26	22
Apr.		Aug.		Dec.	
11	12	4	5	13	7
27	21	20	14	29	16

T VULPEC. 439 *.

(Min. $4^d\ 10\frac{1}{2}^h$.
Max. $1^d\ 9\frac{1}{2}^h$.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
0	12	5	0	3	15
9	9	13	21	12	11
18	6	22	18	21	8
27	3			30	5

T VULPEC (*continu*

Apr.		July	
d	h	d	h
8	2	5	19
16	23	14	16
25	20	23	13
May		Aug.	
4	17	1	10
13	14	10	7
22	11	19	4
31	8	28	1
June		Sept.	
9	5	5	22
18	2	14	19
26	22	23	16

W^5 CYGNI

($6^d\ 4^h$.

Jan.		May	
d	h	d	h
12	13	13	7
Feb.		June	
1	16	2	10
21	19	22	13
Mar.		July	
13	22	12	16
Apr.		Aug.	
3	1	1	19
23	4	21	22

U^4 CYGNI

($5^d\ 3^h$.

Jan.		Mar.	
d	h	d	h
4	19	4	17
19	13	19	11
Feb.		Apr.	
3	6	3	5
18	0	17	22

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* **Alternate minima only are given**: the others can be readily found by adding the interval
the name of the star.

VARIABLES OF SHORT PERIOD (*continued*).

U⁴ CYGNI 448
(*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
15	8	12	6	10	5
30	1	27	0	24	22
Aug.		Oct.		Dec.	
13	19	11	17	9	16
28	13	26	11	24	6

X⁵ CYGNI 452*.
(Min. 7ᵈ 20ʰ·6.
Max. 2ᵈ 2½ʰ.)

Jan.		Mar.		May	
d	h	d	h	d	h
6	20	10	17	12	13
22	13	26	10	28	7
Feb.		Apr.		June	
7	7	11	3	13	0
23	0	26	20	28	17

X⁵ CYGNI 452
(*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
14	10	15	6	1	10
30	3			17	3
Aug.		Oct.		Dec.	
14	20	1	0	2	20
30	13	16	17	18	13

δ CEPHEI 491 *.
(Min. 5ᵈ 8ʰ·8.
Max. 1ᵈ 9⅓ʰ.)

Jan.		Mar.		May	
d	h	d	h	d	h
0	16	6	2	9	11
11	10	16	19	20	5
22	4	27	13	30	22
Feb.		Apr.		June	
1	21	7	7	10	16
12	15	18	0	21	10
23	8	28	18		

δ CEPHE
(*continu*

July		Sept	
d	h	d	
2	3	4	1
12	21	15	
23	14	26	
Aug.		Oct.	
3	8	6	1
14	1	17	11
24	19	28	4

* Alternate minina only are given: the others can be readily found by adding the interval
the name of the star.

MAXIMA OF U PEGASI.

Jan.	Feb.	April.	May.	June.	July.
d h m	d h m	d h m	d h m	d h m	d h m
4 1 45	3 1 20	4 0 30	4 0 5	2 23 40	2 23 16
7 19 42	6 19 17	7 18 27	7 18 2	6 17 37	6 17 12
13 13 39	10 13 14	11 12 24	11 11 59	10 11 34	10 11 9
15 7 36	14 7 11	15 6 21	15 5 56	14 5 31	14 5 6
19 1 32	18 1 8	19 0 18	18 23 53	17 23 28	17 23 3
22 19 29	21 19 4	22 18 15	22 17 50	21 17 25	21 17 0
26 13 26	25 13 1	26 12 12	26 11 47	25 11 22	25 10 57
30 7 23		30 6 8	30 5 44	29 5 19	29 4 54
August.	Sept.	Oct.	Nov.	Dec.	
1 22 51	4 16 23	4 15 58	3 15 33	3 15 8	
5 16 48	8 10 20	8 9 55	7 9 30	7 9 5	
9 10 45	12 4 17	12 3 52	11 3 27	11 3 2	
13 4 31	15 22 13	15 21 49	14 21 24	14 20 59	
16 22 38	19 16 10	19 15 45	18 15 21	18 14 56	
20 16 35	23 10 7	23 9 42	22 9 17	22 8 53	
24 10 32	27 4 4	27 3 39	26 3 14	26 2 49	
28 4 29	30 22 1	30 21 36	29 21 11	29 20 46	
31 22 26					

Every twentieth maximum is given; the others may be found by adding multiples of the period 4ʰ 29ᵐ·8. The minimum takes place 2ʰ 15ᵐ after the maximum.

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1870, observed	19 53 west.	0·1782	67 52
1880, „	18 33 „	0·1804	67 36
1890, „	17 29 „	0·1823	67 23
1900, „	16 29 „	0·1844	67 8
1906, inferred	16 6 „	0·1858	66 54

The horizontal force is given in C. G. S. measure.

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following the Star's name are the Nos. in Catalogue, pp. 23–26.)

U Cephei 18. *Fourth.* $2^d\ 11^h\ 49^{m}{\cdot}7$.

	d	h	m
Jan.	0	16	33
	10	15	52
	20	15	10
	30	14	29
Feb.	9	13	48
	19	13	7
March.	1	12	26
	11	11	45
	21	11	4
	31	10	23
April.	10	9	42
	20	9	1
	30	8	20
May.	10	7	39
	20	6	58
	30	6	17
June.	9	5	36
	19	4	55
	29	4	14
July.	9	3	33
	19	2	52
	29	2	11
August.	8	1	30
	18	0	49
	28	0	8
Sept.	6	23	27
	16	22	46
	26	22	5
Oct.	6	21	24
	16	20	43
	26	20	2
Nov.	5	19	21
	15	18	40
	25	17	59
Dec.	5	17	18
	15	16	37
	25	15	55

Z Persei 46. *Fourth.* $3^d\ 1^h\ 21^{m}{\cdot}5$.

	d	h	m
Jan.	0	10	42
	12	16	9
	24	21	35
Feb.	6	3	1
	18	8	27
March.	2	13	53
	14	19	19
	27	0	45
April.	8	6	12
	20	11	38
May.	2	17	4
	14	22	30
	27	3	56
June.	8	9	22
	20	14	49
July.	2	20	15
	15	1	41
	27	7	7
August.	8	12	33
	20	17	59
Sept.	1	23	25
	14	4	52
	26	10	18
Oct.	8	15	41
	20	21	10
Nov.	2	2	36
	14	8	2
	26	13	28
Dec.	8	18	55
	21	0	21

Algol 51. *Third.* $2^d\ 20^h\ 49^m$.

	d	h	m
Jan.	1	14	59
	10	5	26
	18	19	53
	27	10	20
Feb.	5	0	47
	13	15	13
	22	5	40
March.	2	20	7
	11	10	34
	20	1	1
	28	15	28
April.	6	5	55
	14	20	22
	23	10	48
June.	14	1	30
	22	15	57
July.	1	6	24
	9	20	51
	18	11	17
	27	1	44
August.	4	16	11
	13	6	38
	21	21	5
	30	11	32
Sept.	8	1	59
	16	16	26
	25	6	52
Oct.	3	21	19
	12	11	46
	21	2	13
	29	16	40
Nov.	7	7	7
	15	21	34
	24	12	1
Dec.	3	2	28
	11	16	55
	20	7	21
	28	21	48

T² Persei. 54. *Twelfth.* $0^d\ 20^h\ 23^{m}{\cdot}2$.

	d	h	m
Jan.	0	11	40
	10	16	19
	20	20	57
	31	1	35
Feb.	10	6	14
	20	10	52
March.	2	15	31
	12	20	9
	23	0	47
April.	2	5	26
	12	10	4
	22	14	43
May.	2	19	21
	13	0	0
	23	4	38
June.	2	9	16
	12	13	55
	22	18	33
July.	2	23	12
	13	3	50
	23	8	28
August.	2	13	7
	12	17	45
	22	22	24
Sept.	2	3	2
	12	7	40
	22	12	19
Oct.	2	16	57
	12	21	36
	23	2	14
Nov.	2	6	52
	12	11	31
	22	16	9
Dec.	2	20	48
	13	1	26
	23	6	4

λ Tauri 61. *Second.* $3^d\ 22^h\ 52^m$.

	d	h	m
Jan.	3	8	51
	11	6	36
	19	4	20
	27	2	5
Feb.	3	23	49
	11	21	33
	19	19	18
	27	17	2
March.	7	14	47
	15	12	31
	23	10	15
	31	8	0
April.	8	5	44
	16	3	29
June.	26	7	8
July.	4	4	53
	12	2	37
	20	0	21
	27	22	6
August.	4	19	50
	12	17	35
	20	15	19
	28	13	3
Sept.	5	10	48
	13	8	32
	21	6	17
	29	4	1
Oct.	7	1	45
	14	23	30
	22	21	14
	30	18	59
Nov.	7	16	43
	15	14	27
	23	12	12
Dec.	1	9	56
	9	7	41
	17	5	25
	25	3	9

W² Tauri 62. *Fourth.* $2^d\ 18^h\ 27^{m}{\cdot}2$.

	d	h	m
Jan.	1	17	17
	12	19	6
	23	20	54
Feb.	3	22	43
	15	0	31
	26	2	20
March.	9	4	9
	20	5	57
	31	7	46
April.	11	9	35
	21	11	23
July.	3	11	9
	14	12	58
	25	14	47
August.	5	16	35
	16	18	24
	27	20	12
Sept.	7	22	1
	18	23	50
	30	1	38
Oct.	11	3	27
	22	5	16
Nov.	2	7	4
	13	8	53
	24	10	42
Dec.	5	12	30
	16	14	19
	27	16	7

R Canis Min. 119. *Eighth.* $1^d\ 3^h\ 15^{m}{\cdot}$

	d	h	m
Jan.	0	7	39
	9	9	45
	18	11	51
	27	13	57
Feb.	5	16	3
	14	18	9
	23	20	16
March.	4	22	22
	14	0	28
	23	2	34
April.	1	4	40
	10	6	46
	19	8	52
	28	10	58
May.	7	13	5
	16	15	11
	25	17	17
June.	3	19	23
	12	21	29
August.	1	21	3
	10	23	9
	20	1	15
	29	3	21
Sept.	7	5	27
	16	7	33
	25	9	40
Oct.	4	11	46
	13	13	52
	22	15	58
	31	18	4
Nov.	9	20	10
	18	22	16
	28	0	23
Dec.	7	2	29
	16	4	35
	25	6	41

Minima of Variable Stars of the Algol Type (continued).

Y Camelop.
130.
Second.
$3^{d}\ 7^{h}\ 20^{m}{\cdot}1$.

Jan.
d h m
1 15 15
8 5 55
14 20 35
21 11 15
28 1 56

Feb.
3 16 36
10 7 16
16 21 56
23 12 36

March.
2 3 17
8 17 57
15 8 37
21 23 17
28 13 57

April.
4 4 38
10 19 18
17 9 58
24 0 38
30 15 18

May.
7 5 59
13 20 39
20 11 19
27 1 59

June.
2 16 39
9 7 20
15 22 0
22 12 40
29 3 20

July.
5 18 0
12 8 41
18 23 21
25 14 1

August.
1 4 41
7 19 21
14 10 2
21 0 42
27 15 22

Sept.
3 6 2
9 20 42
16 11 23
23 2 3
29 16 43

Oct.
6 7 23
12 22 3
19 12 44
26 3 24

Y Camelop.
130 (*con.*).

Nov.
d h m
1 18 4
8 8 44
14 23 24
21 14 5
28 4 45

Dec.
4 19 25
11 10 5
18 0 45
24 15 26
31 6 6

R² Puppis
139.
Second.
$6^{d}\ 10^{h}\ 19^{m}{\cdot}5$.

Jan.
d h m
3 23 43
16 20 21
29 17 0

Feb.
11 13 38
24 10 17

March.
9 6 55
22 3 33

April.
4 0 12
16 20 50
29 17 29

May.
12 14 7
25 10 45

June.
7 7 24
20 4 2

July.
3 0 41
15 21 19
28 17 57

August.
10 14 36
23 11 14

Sept.
5 7 53
18 4 31

Oct.
1 1 9
13 21 48
26 18 26

Nov.
8 15 5
21 11 43

Dec.
4 8 21
17 5 0
30 1 38

V Puppis
141.
Eighth.
$1^{d}\ 10^{h}\ 54^{m}{\cdot}4$.

Jan.
d h m
1 3 39
12 18 55
24 10 11

Feb.
5 1 26
16 16 42
28 7 57

March.
11 23 13
23 14 28

April.
4 5 44
15 21 0
27 12 15

May.
9 3 31
20 18 46

June.
1 10 2
13 1 17
24 16 33

July.
6 7 48
17 23 4
29 14 20

August.
10 5 35
21 20 51

Sept.
2 12 6
14 3 22
25 18 37

Oct.
7 9 53
19 1 8
30 16 24

Nov.
11 7 40
22 22 55

Dec.
4 14 11
16 5 26
27 20 42

S Cancri
154.

Jan.
d h m
8 18 20
18 5 58
27 17 36

Feb.
6 5 13
15 16 51
25 4 29

March.
6 16 7
16 3 44
25 15 22

April.
4 3 0
13 14 38
23 2 15

May.
2 13 53
12 1 31
21 13 9
31 0 46

June.
9 12 24
19 0 2
28 11 40

Sept.
2 21 4
12 8 42
21 20 19

Oct.
1 7 57
10 19 35
20 7 13
29 18 50

Nov.
8 6 28
17 18 6
27 5 44

Dec.
6 17 21
16 4 59
25 16 37

S Antliæ
165.
Fortieth.
$0^{d}\ 7^{h}\ 46^{m}{\cdot}8$.

Jan.
d h m
0 11 21
13 10 33
26 9 45

Feb.
8 8 57
21 8 10

March.
6 7 22
19 6 34

April.
1 5 46
14 4 58
27 4 10

May.
10 3 23
23 2 35

June.
5 1 47
18 0 59

July.
1 0 11
13 23 23
26 22 35

Sept.
10 7 48
23 7 0

Oct.
6 6 12
19 5 24

Nov.
1 4 37
14 3 49
27 3 1

Dec.
10 2 13
23 1 25

S Velorum
166.
Second.
$5^{d}\ 22^{h}\ 24^{m}{\cdot}4$.

Jan.
d h m
6 1 54
17 22 43
29 19 32

Feb.
10 16 21
22 13 9

March.
6 9 58
18 6 47
30 3 35

April.
11 0 24
22 21 13

May.
4 18 2
16 14 50
28 11 39

June.
9 8 28
21 5 16

July.
3 2 5
14 22 54
26 19 42

August.
7 16 31
19 13 20
31 10 9

Sept.
12 6 57
24 3 46

Oct.
6 0 35
17 21 23
29 18 12

Nov.
10 15 1
22 11 49

Dec.
4 8 38
16 5 27
28 2 16

W Ursæ Maj. 171.
Sixtieth.
$0^{d}\ 4^{h}\ 0^{m}{\cdot}2$.

Jan.
d h m
0 4 28
10 4 41
20 4 54
30 5 7

Feb.
9 5 20
19 5 34

March.
1 5 47
11 6 0
21 6 13
31 6 27

April.
10 6 40
20 6 53
30 7 6

May.
10 7 20
20 7 33
30 7 46

June.
9 7 59
19 8 12
29 8 26

July.
9 8 39
19 8 52
29 9 5

August.
8 9 19
18 9 32
28 9 45

Sept.
7 9 58
17 10 12
27 10 25

Oct.
7 10 38
17 10 51
27 11 4

Nov.
6 11 18
16 11 31
26 11 44

Dec.
6 11 57
16 12 11
26 12 24

S Antliæ: *20 periods* $= 6^{d}\ 11^{h}\ 36^{m}{\cdot}1$.

W Ursæ Maj.: 30 periods $= 5^{d}\ 0^{h}\ 6^{m}{\cdot}6$.

Minima of Variable Stars of the Algol Type (continued).

R^2 Velorum	Z Draconis	δ Libræ 244.	U Coronæ	R Aræ 292.	U Ophiuchi
183.	193.		250.		306.
Fourth.	*Sixth.*	*Fourth.*	*Third.*	*Second.*	*Tenth.*
$1^{d}\ 20^{h}\ 30^{m}{\cdot}0$.	$1^{d}\ 8^{h}\ 34^{m}{\cdot}7$.	$2^{d}\ 7^{h}\ 51^{m}{\cdot}4$.	$3^{d}\ 10^{h}\ 51^{m}{\cdot}2$.	$4^{d}\ 10^{h}\ 12^{m}{\cdot}1$.	$0^{d}\ 20^{h}\ 7^{m}{\cdot}7$.
Jan.	Jan.	Jan.	Jan.	Jan.	Jan.
d h m	d h m	d h m	d h m	d h m	d h m
0 22 28	1 6 28	0 8 27	3 11 58	0 19 28	0 23 53
8 8 28	9 9 57	9 15 52	13 20 32	9 15 53	9 9 10
15 18 28	17 13 25	18 23 18	24 5 5	18 12 17	17 18 27
23 4 28	25 16 53	28 6 43	Feb.	27 8 41	26 3 43
30 14 29	Feb.	Feb.	3 13 39	Feb.	Feb.
Feb.	2 20 22	6 14 9	13 22 13	5 5 5	3 13 0
7 0 29	10 23 50	15 21 34	24 6 47	14 1 30	11 22 17
14 10 29	19 3 18	25 5 0	March.	22 21 54	20 7 34
21 20 29	27 6 47	March.	6 15 20	March.	28 16 51
March.	March.	6 12 25	16 59 54	3 18 18	March.
1 6 29	7 10 15	15 19 51	27 8 28	12 14 42	9 2 8
8 16 29	15 13 43	25 3 16	April.	21 11 7	17 11 25
16 2 30	23 17 11	April.	6 17 2	30 7 31	25 20 42
23 12 30	31 20 40	3 10 42	17 1 35	April.	April.
30 22 30	April.	12 18 7	27 10 9	8 3 55	3 5 59
April.	9 0 8	22 1 33	May.	17 0 20	11 15 16
7 8 30	17 3 36	May.	7 18 43	25 20 44	20 0 32
14 18 30	25 7 5	1 8 58	18 3 17	May.	28 9 49
22 4 30	May.	10 16 24	28 11 50	4 17 8	May.
29 14 30	3 10 33	19 23 50	June.	13 13 32	6 19 6
May.	11 14 1	29 7 15	7 20 24	22 9 57	15 4 23
7 0 31	19 17 30	June.	18 4 58	31 6 21	23 13 40
14 10 31	27 20 58	7 14 41	28 13 32	June.	31 22 57
21 20 31	June.	16 22 6	July.	9 2 45	June.
29 6 31	5 0 26	26 5 32	8 22 5	17 23 9	9 8 14
June.	13 3 54	July.	19 6 39	26 19 34	17 17 31
5 16 31	21 7 23	5 12 57	29 15 13	July.	26 2 48
13 2 32	29 10 51	14 20 23	August.	5 15 58	July.
20 12 32	July.	24 3 48	8 23 47	14 12 22	4 12 4
27 22 32	7 14 19	August.	19 8 20	23 8 46	12 21 21
July.	15 17 48	2 11 14	29 16 54	August.	21 6 38
5 8 32	23 21 16	11 18 39	Sept.	1 5 11	29 15 55
12 18 32	August.	21 2 5	9 1 28	10 1 35	August.
20 4 32	1 0 44	30 9 30	19 10 2	18 21 59	7 1 12
27 14 33	9 4 13	Sept.	29 18 35	27 18 23	15 10 29
August.	17 7 41	8 16 56	Oct.	Sept.	23 19 46
4 0 33	25 11 9	18 0 21	10 3 9	5 14 48	Sept.
11 10 33	Sept.	27 7 47	20 11 43	14 11 12	1 5 3
18 20 33	2 14 37	Dec.	30 20 16	23 7 36	9 14 20
26 6 33	10 18 6	1 11 46	Nov.	Oct.	17 23 37
Sept.	18 21 34	10 19 11	10 4 50	2 4 1	26 8 53
2 16 33	27 1 2	20 2 37	20 13 24	11 0 25	Oct.
10 2 34	Oct.	29 10 2	30 21 58	19 20 49	4 18 10
17 12 34	5 4 31		Dec.	28 17 13	13 3 27
24 22 34	13 7 59		11 6 31	Nov.	21 12 44
Oct.	21 11 27		21 15 5	6 13 38	29 22 1
2 8 34	29 14 56		31 23 39	15 10 2	Nov.
9 18 34	Nov.			24 6 26	7 7 18
17 4 34	6 18 24			Dec.	15 16 35
24 14 34	14 21 52			3 2 50	
Nov.	23 1 20			11 23 15	
1 0 35	Dec.			20 19 39	
8 10 35	1 4 49			29 16 3	
15 20 35	9 8 17				
23 6 35	17 11 45				
30 16 35	25 15 14				
Dec.					
8 2 35					
15 2 36					
22 22 36					
30 8 36					

Minima of Variable Stars of the Algol Type (continued).

Z Herculis	S^2 Sagittarii	V Serpentis	X^2 Herculis	R^2 Draconis	U Scuti
320.	327.	328.	335.	344.	351.
Fourth.	*Fourth.*	*Fourth.*	*Twelfth.*	*Fourth.*	*Tenth.*
	$2^{d}\,9^{h}\,58^{m}.6$.	$3^{d}\,10^{h}\,53^{m}.0$.	$0^{d}\,21^{h}\,20^{m}.6$.	$2^{d}\,19^{h}\,56^{m}.8$.	$0^{d}\,22^{h}\,55^{m}.2$.
Jan.	Jan.	Feb.	Jan.	Jan.	Feb.
d h m	d h m	d h m	d h m	d h m	d h m
2 7 38	18 2 55	10 0 46	11 10 41	0 4 54	7 4 53
10 7 17	27 18 49	23 20 18	22 2 48	11 12 42	16 18 5
18 6 56	Feb.	March.	Feb.	22 20 29	26 7 17
26 6 36	6 10 44	9 15 50	1 18 55	Feb.	March.
Feb.	16 2 38	23 11 22	12 11 2	3 4 16	7 20 29
3 6 15	25 18 33	April.	23 3 9	14 12 3	17 9 40
11 5 54	March.	6 6 54	March.	25 19 50	26 22 52
19 5 33	7 10 27	20 2 26	5 19 15	March.	April.
27 5 12	17 2 21	May.	16 11 22	9 3 38	5 12 4
March.	26 18 16	3 21 58	27 3 29	20 11 25	15 1 15
7 4 51	April.	17 17 30	April.	31 19 12	24 14 27
15 4 30	5 10 10	31 13 2	6 19 36	April.	May.
23 4 9	15 2 5	June.	17 11 43	12 2 59	4 3 39
31 3 48	24 17 59	14 8 34	28 3 50	23 10 46	13 16 50
April.	May.	28 4 4	May.	May.	23 6 2
8 3 28	4 9 54	July.	8 19 57	4 8 34	June.
16 3 7	14 1 48	11 23 38	19 12 4	16 2 21	1 19 14
24 2 46	23 17 43	25 19 10	30 4 11	27 10 8	11 8 26
May.	June.	August.	June.	June.	20 21 37
2 2 25	2 9 37	8 14 42	9 20 18	7 17 55	30 10 49
10 2 4	12 1 32	22 10 14	20 12 24	19 1 42	July.
18 1 43	21 17 26	Sept.	July.	30 9 30	10 0 1
26 1 22	July.	5 5 46	1 4 31	July.	19 13 13
June.	1 9 20	19 1 18	11 20 38	11 17 17	29 2 24
3 1 1	11 1 15	Oct.	22 12 45	23 1 4	August.
11 0 40	20 17 9	2 20 51	August.	August.	7 15 36
19 0 20	30 9 4	16 16 23	2 4 52	3 8 51	17 4 48
26 23 59	August.	30 11 55	12 20 59	14 16 38	26 17 59
July.	9 0 58	Nov.	23 13 6	26 0 26	Sept.
4 23 38	18 16 53	13 7 27	Sept.	Sept.	5 7 11
12 23 17	28 8 47		3 5 13	6 8 13	14 20 23
20 22 56	Sept.		13 21 20	17 16 0	24 9 35
28 22 35	7 0 42		24 13 27	28 23 47	Oct.
August.	16 16 36		Oct.	Oct.	3 22 46
5 22 14	26 8 21		5 5 33	10 7 34	13 11 57
13 21 53	Oct.		15 21 40	21 15 21	23 1 9
21 21 32	6 0 25		26 13 47	Nov.	Nov.
29 21 12	15 16 19		Nov.	1 23 9	1 14 21
Sept.	25 9 14		6 5 54	13 6 56	11 3 32
6 20 51	Nov.		16 22 1	24 14 43	20 16 45
14 20 30	4 0 8		27 14 8	Dec.	30 5 56
22 20 9	13 16 3		Dec.	5 22 30	
30 19 48	23 7 57		8 6 15	17 6 17	
Oct.	Dec.		18 22 22	28 14 5	
8 19 27	2 23 52		29 14 29		
16 19 6					
24 18 45					
Nov.					
1 18 24					
9 18 3					
17 17 43					
25 17 22.					
Dec.					
3 17 1					
11 16 40					
19 16 19					
27 15 58					

Z Herculis: 1 *period* $=1^{d}\,22^{h}\,28^{m}$; 2 periods $=3^{d}\,23^{h}\,50^{m}$; 3 periods $=5^{d}\,22^{h}\,17^{m}$.

Minima of Variable Stars of the Algol Type (continued).

V^2 LYRÆ 367.
Fourth.
$3^d\ 14^h\ 22^m\cdot4$.

Jan.
d h m
2 9 45
16 19 15
31 4 44
Feb.
14 14 14
28 23 44
March.
15 9 13
29 18 43
April.
13 4 12
27 13 42
May.
11 23 11
26 8 41
June.
9 18 11
24 3 40
July.
8 13 10
22 22 39
August.
6 8 9
20 17 38
Sept.
4 3 8
18 12 37
Oct.
2 22 7
17 7 37
31 17 6
Nov.
15 2 36
29 12 5
Dec.
13 21 35
28 7 4

U SAGITTÆ 370.
Fourth.
$3^d\ 9^h\ 8^m\cdot2$.

Jan.
d h m
1 2 26
14 14 59
28 3 32
Feb.
10 16 6
24 4 39
March.
9 17 12
23 5 45
April.
5 18 18
19 6 52
May.
2 19 25
16 7 58
29 20 31
June.
12 9 4
25 21 38
July.
9 10 11
22 22 44
August.
5 11 17
18 23 56
Sept.
1 12 24
15 0 57
28 13 30
Oct.
12 2 3
25 14 36
Nov.
8 3 10
21 15 43
Dec.
5 4 16
18 16 49

X^3 CYGNI 383.
Second.
$6^d\ 0^h\ 8^m\cdot8$.

Jan.
d h m
4 17 17
16 17 34
28 17 52
Feb.
9 18 10
21 18 27
March.
5 18 45
17 19 2
29 19 20
April.
10 19 38
22 19 55
May.
4 20 13
16 20 30
28 20 48
June.
9 21 6
21 21 23
July.
3 21 41
15 21 58
27 22 16
August.
8 22 34
20 22 51
Sept.
1 23 9
13 23 26
25 23 44
Oct.
8 0 2
20 0 19
Nov.
1 0 37
13 0 54
25 1 12
Dec.
7 1 30
19 1 47
31 2 5

Z^1 CYGNI 397.
Fourth.
$3^d\ 7^h\ 37^m\cdot9$.

Jan.
d h m
3 2 8
16 8 40
29 15 11
Feb.
5 6 27
18 12 58
March.
3 19 30
17 2 1
30 8 33
April.
12 15 5
25 21 36
May.
9 4 8
22 10 39
June.
4 17 11
17 23 43
July.
1 6 14
14 12 46
27 19 17
August.
10 1 49
23 8 21
Sept.
5 14 52
18 21 24
Oct.
2 3 55
15 10 27
28 16 59
Nov.
10 23 30
24 6 2
Dec.
7 12 33
20 19 5

V^3 CYGNI 399.
Second.
$4^d\ 13^h\ 45^m\cdot0$.

Jan.
d h m
0 4 29
9 7 59
18 11 30
27 15 0
Feb.
5 18 30
14 22 0
24 1 30
March.
5 5 0
14 8 30
23 12 0
April.
1 15 30
10 19 0
19 22 30
29 2 0
May.
8 5 30
17 9 0
26 12 30
June.
4 16 1
13 19 31
22 23 1
July.
2 2 31
11 6 1
20 9 31
29 13 1
August.
7 16 31
16 20 1
25 23 32
Sept.
4 3 1
13 6 31
22 10 1
Oct.
1 13 31
10 17 1
19 20 32
29 0 2
Nov.
7 3 32
16 7 2
25 10 32
Dec.
4 14 2
13 17 32
22 21 2

V^5 CYGNI 412.
Second.
$8^d\ 10^h\ 23^m\cdot0$.

Jan.
d h m
6 15 51
23 12 37
Feb.
9 9 23
26 6 9
March.
15 2 55
31 23 41
April.
17 20 27
May.
4 17 13
21 13 59
June.
7 10 45
24 7 31
July.
11 4 17
28 1 3
August.
13 21 49
30 18 35
Sept.
16 15 21
Oct.
3 12 7
20 8 53
Nov.
6 5 39
23 2 25
Dec.
9 23 11
26 19 57

Minima of Variable Stars of the Algol Type (continued).

Z¹ CYGNI 416. *Fourth.* $3^d\ 10^h\ 49^m{\cdot}2$.

	d	h	m
Jan.	1	22	26
	15	17	43
	29	12	59
Feb.	12	8	16
	26	3	33
March.	11	22	50
	25	18	7
April.	8	13	23
	22	8	40
May.	6	3	57
	19	23	14
June.	2	18	30
	16	13	47
	30	9	4
July.	14	4	21
	27	23	38
August.	10	18	54
	24	14	11
Sept.	7	9	28
	21	4	45
Oct.	5	0	1
	18	19	18
Nov.	1	14	35
	15	9	52
	29	5	9
Dec.	13	0	25
	26	19	42

W DELPHINI 424. *Second.* $4^d\ 19^h\ 20^m{\cdot}9$.

	d	h	m
Jan.	1	22	40
	11	13	22
	21	4	4
	30	18	45
Feb.	9	9	27
	19	0	9
	28	14	51
March.	10	5	32
	19	20	14
	29	10	56
April.	8	1	38
	17	16	19
	27	7	1
May.	6	21	43
	16	12	24
	26	3	6
June.	4	17	48
	14	8	30
	23	23	11
July.	3	13	53
	13	4	35
	22	19	17
August.	1	9	58
	11	0	40
	20	15	22
	30	6	4
Sept.	8	20	45
	18	11	27
	28	2	9
Oct.	7	16	50
	17	7	32
	26	22	14
Nov.	5	12	56
	15	3	37
	24	18	19
Dec.	4	9	1
	13	23	43
	23	14	24

Y CYGNI 441. *Sixth.*

	d	h	m
Jan.	1	21	57
	10	21	42
	19	21	27
	28	21	13
Feb.	6	20	58
	15	20	43
	24	20	28
March.	5	20	13
	14	19	58
	23	19	43
April.	1	19	28
	10	19	13
	19	18	58
	28	18	44
May.	7	18	29
	16	18	14
	25	17	59
June.	3	17	44
	12	17	29
	21	17	14
	30	16	59
July.	9	16	44
	18	16	29
	27	16	15
August.	5	16	0
	14	15	45
	23	15	30
Sept.	1	15	15
	10	15	0
	19	14	45
	28	14	31
Oct.	7	14	16
	16	14	1
	25	13	46
Nov.	3	13	31
	12	13	16
	21	13	2
	30	12	47
Dec.	9	12	32
	18	12	17
	27	12	2

U⁵ CYGNI 455. *Eighth* $1^d\ 11^h\ 27^m{\cdot}0$.

	d	h	m
Jan.	0	23	51
	12	19	26
	24	15	2
Feb.	5	10	38
	17	6	13
March.	1	1	49
	12	21	44
	24	17	0
April.	5	12	36
	17	8	11
	29	3	47
May.	10	23	22
	22	18	57
June.	3	14	34
	15	10	9
	27	5	45
July.	9	1	21
	20	20	56
August.	1	16	32
	13	12	8
	25	7	43
Sept.	6	3	19
	17	22	54
	29	18	30
Oct.	11	14	6
	23	9	41
Nov.	4	5	17
	16	0	53
	27	20	28
Dec.	9	16	4
	21	11	39

Y⁵ CYGNI 476.

	d	h	m
Jan.	5	5	34
	14	23	1
	24	16	28
Feb.	3	9	55
	13	3	22
	22	20	49
March.	4	14	16
	14	7	43
	24	1	9
April.	2	18	36
	12	12	3
	22	5	30
May.	1	22	57
	11	16	24
	21	9	51
	31	3	18
June.	9	20	44
	19	14	11
	29	7	38
July.	9	1	5
	18	18	32
	28	11	59
August.	7	5	26
	16	22	53
	26	16	20
Sept.	5	9	46
	15	3	13
	24	20	40
Oct.	4	14	7
	14	7	34
	24	1	1
Nov.	2	8	28
	12	11	55
	22	5	21
Dec.	1	22	48
	11	16	15
	21	9	42
	31	3	9

T⁵ CYGNI 477.

	d	h	m
Jan.	4	11	17
Feb.	4	18	35
March.	8	1	53
April.	8	9	10
May.	9	16	28
June.	9	23	46
July.	11	7	4
Aug.	11	14	21
Sept.	11	21	39
Oct.	13	4	57
Nov.	13	12	15
Dec.	14	19	33

Y Cygni: other minima follow those given by $1^d\ 12^h\ 35^m$, $2^d\ 23^h\ 54^m$, $4^d\ 12^h\ 29^m$, $5^d\ 23^h\ 50^m$, and $7^d\ 12^h\ 23^m$ (in January), by $1^d\ 11^h\ 32^m$, $2^d\ 23^h\ 55^m$, $4^d\ 11^h\ 16^m$,

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to the Catalogue on pp. 23-26; M signifies maximum; *m*, minimum.

nuary.

	No.	
	262	M
	290	*m*
	77	*m*
3	386	M
	300	M
	514	*m*
4	39	M
	128	M
	72	*m*
5	52	M
	11	M
7	407	M
	251	M
	194	*m*
	167	*m*
	93	*m*
	384	*m*
	353	*m*
8	71	M
	70	M
	17	*m*
9	346	M
	405	M
	379	*m*
	428	*m*
1	488	M
	74	M
	314	*m*
2	401	M
	305	M
	38	M
	26	M
	338	*m*
3	414	M
	352	M
	462	M
	385	M
	196	M
4	115	M
	387	M
	53	M
	517	M
5	98	*m*
	223	*m*
6	515	M
	143	*m*
8	77	M
	288	M
	8	*m*
	97	*m*
9	25	M
	31	*m*
0	136	M
	380	M
	204	M
1	361	M
	396	M
2	220	M
	37	M
	24	*m*

January (*con.*).

d	No.	
23	100	M
	175	*m*
	247	*m*
25	337	M
	459	M
	36	M
	502	M
	56	M
26	60	*m*
	100	*m*
	33	*m*
27	191	M
28	234	M
	211	M
	423	*m*
29	216	M
	143	M
	58	*m*
	354	*m*
	230	*m*
30	353	M
	178	M
	147	*m*
31	404	M
	190	M
	293	*m*
	82	*m*
	314	*m*

February.

d	No.	
1	128	*m*
	160	*m*
2	273	M
3	340	M
	236	M
	59	*m*
	27	*m*
	77	*m*
4	283	M
	275	M
	68	M
	181	M
	257	*m*
5	194	M
	175	M
	363	M
	400	M
	512	M
6	167	M
	226	M
	456	*m*
	254	*m*
7	10	M
	214	*m*
8	280	M
	256	*m*
	109	*m*
9	237	M

February (*con.*).

d	No.	
9	65	M
	495	M
	321	*m*
10	137	M
	490	*m*
	272	*m*
	266	*m*
11	267	M
	290	M
	509	M
	239	*m*
12	284	M
	279	M
	5	*m*
13	482	*m*
14	241	M
	1	M
	192	*m*
15	406	M
16	89	M
	468	M
	425	M
	410	M
17	180	M
	45	M
18	13	M
	125	M
	299	M
	79	*m*
19	326	M
	207	M
	128	M
	264	M
	77	M
20	500	M
21	55	M
	378	M
	314	*m*
	302	*m*
22	291	M
	466	*m*
	409	*m*
	329	*m*
23	253	M
	29	M
	353	*m*
	385	*m*
24	32	*m*
	50	*m*
26	240	M
	243	*m*
	143	*m*
27	248	M
	175	*m*
28	314	M
	341	M
	379	M
	390	*m*
	463	*m*
	112	*m*

March.

d	No.	
1	66	M
	407	*m*
2	140	M
	95	M
	451	M
	194	*m*
3	148	M
4	197	*m*
5	473	M
	434	*m*
	190	*m*
	19	*m*
7	294	M
	393	M
	311	M
8	221	M
	492	*m*
	77	*m*
	438	*m*
	273	*m*
9	98	M
	67	M
10	232	M
	354	M
	167	*m*
	265	*m*
11	190	M
	282	M
	143	M
	286	*m*
12	58	M
	245	M
	175	M
	372	*m*
	126	*m*
13	142	M
	357	M
	314	*m*
	415	*m*
14	210	M
	177	M
	81	*m*
	381	*m*
15	189	M
	442	*m*
16	488	*m*
17	353	M
18	14	M
	407	M
	117	*m*
19	186	M
	411	M
	128	*m*
20	388	M
	314	M
21	385	M
	121	*m*
22	478	M
24	77	M
	331	M

March (*con.*).

d	No.	
24	307	*m*
25	15	M
26	220	*m*
27	296	M
28	401	*m*
	462	*m*
29	156	M
	16	*m*
31	194	M
	295	M
	63	M
	129	*m*
	34	*m*

April.

d	No.	
1	355	M
	315	M
	368	*m*
	392	*m*
	206	*m*
2	384	M
	322	M
	43	M
	203	*m*
	314	*m*
3	209	M
	464	*m*
4	175	*m*
	375	*m*
5	515	*m*
	3	*m*
6	72	M
	128	M
7	188	M
8	398	M
	278	M
	57	M
	467	*m*
9	167	M
	314	M
	143	*m*
	77	*m*
	195	*m*
10	99	M
	152	M
	86	M
	111	*m*
	499	*m*
	353	*m*
11	119	M
	501	*m*
	503	*m*
12	325	*m*
	290	*m*
13	168	M
	190	*m*
	423	*m*

April (*con.*).

d	No.	
14	454	M
	334	M
15	419	M
16	30	M
	458	*m*
	209	*m*
	251	*m*
17	496	M
	175	M
18	246	M
	100	*m*
	137	*m*
19	414	M
	190	M
	41	*m*
	38	*m*
	25	*m*
20	313	M
	309	*m*
21	88	M
	333	M
22	143	M
	489	M
	220	M
	472	*m*
23	314	*m*
24	104	*m*
	218	*m*
25	77	M
	345	M
	192	M
	445	M
	257	M
	468	*m*
26	272	M
27	33	M
	444	M
	429	M
	205	*m*
28	409	M
	47	*m*
	12	*m*
	354	*m*
29	504	M
	340	*m*
	53	*m*
	172	*m*
30	314	M

May.

d	No.	
1	372	M
	480	M
	58	*m*
	28	*m*
	385	*m*
2	425	*m*
	180	*m*

May (*con.*).

d	No.	
3	353	M
	174	M
4	348	M
	361	*m*
	191	*m*
	128	*m*
5	508	*m*
	323	*m*
6	303	M
	379	*m*
	196	*m*
7	273	M
	71	*m*
	207	*m*
8	247	M
	103	M
	98	*m*
9	115	M
	194	*m*
	175	*m*
11	362	M
	9	M
	308	M
	167	*m*
	407	*m*
	77	*m*
12	162	M
	405	*m*
	204	*m*
13	466	M
	314	*m*
14	7	M
15	418	M
	279	*m*
	451	*m*
	326	*m*
16	60	M
	82	M
17	262	*m*
18	8	M
	169	*m*
19	120	M
	420	M
	486	M
	121	M
20	314	M
	143	*m*
21	261	M
	56	*m*
22	89	M
	298	M
	492	M
	290	M
	128	M
	175	M
	190	*m*
24	482	M
	329	M
	239	M
	510	*m*

May (*con.*).

d	No.	
25	157	M
	460	M
	101	*m*
26	147	M
	484	M
	518	M
	211	*m*
	353	*m*
27	77	M
	190	M
28	140	M
	488	M
	407	M
	385	M
	214	M
29	287	M
	417	M
	19	M
30	21	M
	302	M
31	474	M

June.

d	No.	
1	463	M
	243	M
2	42	M
	143	M
	314	*m*
3	453	M
	490	M
4	438	M
	456	M
	187	M
	300	*m*
5	516	M
	11	*m*
6	22	M
	159	M
	179	M
	432	M
7	194	M
	133	M
	59	M
	266	M
	354	M
	10	*m*
	283	*m*
	114	*m*
8	35	M
	481	M
	401	M
	293	M
	4	M
9	314	M
	450	*m*
	87	*m*

J
(

d
9
10
11
12
13
14
15
16
17
18
19
21
22
23
24
25
26
27
28
29

Maxima and Minima of Variable Stars (continued).

June (*con.*).

No.	
91	M
314	M
340	M
241	m
145	m
509	m
190	m
143	m

July.

No.	
34	M
96	M
97	M
	M
	M
436	M
00	M
440	M
514	M
425	M
190	M
222	m
128	M
110	M
385	m
138	M
258	M
6	M
443	M
223	M
325	M
480	M
25	M
136	m
23	m
167	m
353	m
45	m
213	M
377	M
494	M
143	M
314	m
221	m
207	M
53	M
230	M
180	M
77	m
83	M
451	M
392	M
39	m
219	M
33	M
7	M
	M
	M

July (*con.*).

d	No.	
18	251	M
	257	m
20	314	M
	467	M
	194	m
	175	m
	407	m
21	205	M
	220	M
	290	m
22	126	M
	462	M
	294	m
23	410	m
24	414	M
25	43	m
26	359	M
	272	m
	354	m
2	501	M
	369	m
	49	m
	33	m
2	449	M
	192	m
	209	m
29	296	m
30	442	M
	502	m
31	77	M
	488	m

August.

d	No.	
1	58	m
	76	m
2	175	M
	326	M
	80	M
	135	m
	314	m
	322	m
3	450	M
	385	M
	35	M
4	17	M
	96	M
	92	M
	28	m
5	191	M
	309	M
	26	m
6	407	M
	395	M
	12	M
7	465	M
	90	M
	284	M
	190	m
8	255	M
	338	M
	195	M

August (*con.*).

d	No.	
8	265	M
	273	M
9	32	M
	511	M
	314	M
	405	M
	121	m
	363	m
10	321	M
	28	M
	384	m
	74	m
	167	M
	263	M
	143	m
3	190	M
5	37	m
6	161	M
	77	m
17	428	M
	137	m
8	194	M
	475	M
	458	M
	203	M
	358	m
9	111	M
20	356	M
21	401	m
22	140	M
	211	M
	128	M
	243	m
	160	m
23	314	m
	264	m
24	461	M
	143	M
	372	m
	175	m
2	89	M
	56	M
26	204	M
2	459	M
	361	M
	503	M
2	268	M
	98	m
30	314	M
	464	M
	290	M
	10	M
	427	M
	118	M
	297	M
	379	m

September.

d	No.	
1	115	M
	341	M
	77	M

September (*con.*).

d	No.	
1	270	m
	156	m
2	409	M
	3	M
	47	M
	262	M
3	479	M
	498	M
	286	M
	468	m
4	375	M
	365	M
	354	M
5	387	m
6	41	M
	487	M
	175	M
	515	m
	411	m
	329	m
7	299	m
8	307	M
	366	m
9	302	m
10	408	m
11	406	M
	226	M
	129	M
	482	m
	423	m
	273	m
12	58	M
	167	m
	66	m
	314	m
13	82	m
	385	m
14	88	M
15	227	M
	254	m
	190	m
16	185	M
	170	M
	456	m
	398	m
17	57	M
	242	M
	237	M
	352	M
	30	m
18	454	M
	194	m
	77	m
	425	m
	247	m
19	353	M
	314	M
	16	M
	259	M
	128	m
20	289	M
	463	m

September (*con.*).

d	No.	
20	381	m
21	190	M
	380	M
	143	m
22	279	M
	220	m
	357	m
23	319	M
	72	M
	466	m
	340	m
25	40	M
26	478	m
	438	m
27	483	M
	100	m
	451	m
28	102	M
	151	M
	304	M
	183	m
	186	m
	407	m
29	218	M
	499	M
	29	M
	175	m
30	283	M
	44	M
	59	m
	207	m
	50	m

October.

d	No.	
1	104	M
	444	m
2	178	m
	314	m
	266	m
	275	M
	38	m
4	77	M
	143	M
	60	m
	34	m
	429	m
	462	m
	19	m
6	257	M
	200	M
	192	M
	216	M
	490	m
7	121	M
	323	M
	240	M
	128	M
8	176	M
9	272	M

October (*con.*).

d	No.	
9	314	M
	25	m
10	508	M
	385	M
	137	M
	492	m
11	172	M
	252	M
	267	M
12	488	M
	109	M
	167	M
	175	M
	300	M
	113	m
13	372	M
	469	M
	396	M
	470	M
	353	m
14	291	M
	168	m
15	371	M
	407	M
16	169	M
17	194	M
18	108	M
	274	M
19	360	M
	420	m
	174	m
20	98	M
	220	M
	379	M
	77	m
21	282	M
	253	m
22	234	M
	500	M
23	71	m
	314	m
24	278	M
	512	M
	384	M
	354	m
	190	m
	298	m
25	7	m
	239	m
	251	m
26	33	M
	285	M
	326	m
27	241	M
28	414	M
	64	M
	415	M
29	24	M
	190	M
	290	m
	147	m
	53	m

October (*con.*).

d	No.	
30	314	M
	127	m
	467	m
	110	m

November.

d	No.	
1	430	M
	55	M
	401	M
	58	m
	143	m
2	236	M
	9	m
3	404	M
	145	M
	409	m
	175	m
4	81	M
	353	M
5	77	M
	119	m
	434	m
	128	m
	63	m
6	468	M
	194	m
7	509	M
	294	M
	293	m
9	333	M
	136	M
	191	m
10	273	M
	322	M
	388	m
11	221	M
	68	M
	199	M
12	45	M
	473	M
	314	m
	79	m
13	224	M
	209	M
	167	m
14	143	M
15	86	m
16	140	M
	229	M
	175	M
17	4	M
18	87	M
	378	m
19	314	M
20	515	M
	368	m
	385	m
	460	m
	5	m

November (*con.*).

d	No.	
21	425	M
	77	m
23	128	M
	2	M
	36	M
24	340	M
	43	M
25	243	M
	196	m
	158	m
	423	m
26	248	M
	93	m
	120	m
27	78	M
28	89	M
	451	M
	325	m
	353	m
	501	m
29	392	m
30	15	M
	410	M
	235	M
	103	m

December.

d	No.	
1	173	M
2	256	M
	65	m
	205	m
	190	m
	314	m
3	354	M
	31	m
4	109	M
5	194	M
	69	M
	214	m
6	14	M
	495	M
	329	M
	57	m
7	1	M
	11	M
	190	M
	77	M
	207	M
8	290	M
	422	M
	361	m
	407	m
9	314	M
	175	m
10	113	M
	156	M
	405	m
	72	m
11	71	M
	180	M
12	38	M

Decen (*con*

d	No
12	46
	17
13	5
	16
	10
	32
	143
	126
	273
	338
14	510
	67
	223
15	49
	484
	488
16	302
	385
	204
	137
17	146
	152
	386
	381
	206
18	403
	211
19	98
20	482
	37
	308
	341
	56
	442
21	353
	220
	128
22	175
	463
	279
	314
23	23
	438
	210
	77
24	492
25	114
	407
26	143
	246
	379
27	82
	121
28	464
	257
29	314
	489
	111
30	10
31	159
	33

DOUBLE STARS.

Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1900+.	Observer.
	h m	° ′		°	″		
062	0 0·9	+57 52	6½ ; 7½	348·1	1·48	4·00	B.
12, λ Cassiop.	0 26·4	+53 58	6½ ; 6½	153·8	0·50	3·96	B.
95	0 32·2	−25 19	6·5 ; 6·7	293·6	0·57	2·80	D.
95	0 43·4	+18 9	8 ; 8	219·9	0·81	4·00	B.
0, η Cassiop.	0 43·0	+57 17	3½ ; 7½	230·9	5·57	5·08	M.
20, 66 Piscium	0 49·2	+18 29	6 ; 6	315·0	0·43	3·54	W.B.
3, 36 Andromedæ	0 50·1	+23 2	6 ; 6½	27·4	1·22	5·08	M.
13, 42 Ceti	1 14·3	− 1 3	6½ ; 7	352·4	1·25	2·99	W.B.
036	1 15·0	−16 19	7 ; 7½	11·3	1·15	3·93	B.
ridani	1 35·7	−56 42	6 ; 6	223·0	8·12	1·05	T.
38, γ Androm. BC.	1 57·8	+41 51	5½ ; 6	113·8	0·45	3·81	Ls.
05, γ Androm. AB.	1 57·8	+41 51	2½ ; 5½	61·8	10·20	3·81	Ls.
28	2 7·3	+56 59	6½ ; 7½	90·8	0·65	3·99	B.
05	2 41·8	+18 58	7½ ; 8	311·8	3·00	5·04	W.B.
33, ε Arietis	2 53·5	+20 56	5½ ; 6	198·0	1·11	5·05	W.B.
25	2 53·5	+21 13	7½ ; 7½	129·0	0·28	3·33	Ls.
67	3 8·9	+ 0 22	8 ; 8½	205·4	0·57	4·15	B.
53	3 11·3	+33 16	7 ; 8	228·4	0·64	3·20	Ls.
12, 7 Tauri	3 28·5	+24 8	6½ ; 7	4·6	0·25	3·47	W.B.
25	3 33·8	+33 49	7½ ; 8	271·1	2·42	4·04	W.B.
531	4 0·8	+37 49	6½ ; 8½	134·4	1·69	3·20	Ls.
82	4 17·1	+14 49	7 ; 9	110·2	0·65	4·18	B.
83, AB	4 45·6	+10 54	7½ ; 8	90·9	0·24	3·05	Ls.
AC	......	......	7½ ; 10	157·6	17·60	3·06	Ls.
ssey 445	4 55·8	+20 41	8½ ; 8½	290·4	0·47	4·21	Ls.
238	4 55·2	+26 24	8 ; 11½	12·4	1·41	4·17	Ls.
98, ι Orionis	5 2·4	+ 8 23	6 ; 6½	169·5	0·92	4·10	B.
42, 380 Tauri	5 30·4	+21 56	7 ; 7½	259·7	3·52	3·29	B.
49	5 30·9	+26 53	6½ ; 7½	169·4	0·73	3·21	B.
32	6 28·5	+14 49	8 ; 8	327·2	1·94	4·05	B.
149	6 30·0	+27 22	6½ ; 9	265·9	0·64	3·17	Ls.
48, 12 Lyncis AB	6 37·3	+59 33	6 ; 6½	117·2	1·51	2·22	M.
us	6 40·6	−16 34	1 ; 10	112·9	5·82	4·19	Ls.
156	6 41·6	+18 18	6½ ; 7	298·3	0·50	3·12	W.B.
037	7 6·6	+27 24	7 ; 7	300·4	0·75	4·15	B.
170	7 12·2	+ 9 29	7 ; 7½	107·4	1·62	5·16	Ls.
066, δ Geminor.	7 14·1	+22 10	3 ; 8	208·4	6·99	3·21	B.
79	7 27·9	+33 20	8 ; 10½	215·6	1·04	4·26	B.
110, Castor	7 28·2	+32 7	2 ; 3	224·8	5·61	3·09	Dob.
ocyon	7 34·0	+ 5 29	1 ; 8½	344·9	5·39	2·21	Ls.
187	8 3·2	+32 31	7 ; 8	222·9	2·13	3·27	B.
196, ζ Cancri AB	8 6·4	+17 59	5½ ; 7½	347·0	1·18	5·34	M.
” ” AC	......	......	5½ ; 6½	111·1	5·00	5·34	M.
” ” BC	......	......	7½ ; 6½	121·1	6·13	5·23	W.B.
216	8 16·3	− 1 17	7½ ; 8	195·3	0·46	4·23	B.
ugh 354	8 36·6	+26 4	8 ; 8½	182·8	1·00	4·26	Ls.
273, ε Hydræ AB	......	......	4 ; 6	147·0	0·23	4·24	Ls.
273, ε Hydræ AC	8 41·5	+ 6 50	4 ; 7	232·2	3·51	4·24	Ls.
121	9 11·9	+29 0	7½ ; 7½	25·5	0·75	3·25	B.
201	9 18·0	+28 20	7 ; 11	221·2	1·52	3·18	W.B.
1356, ω Leonis	9 23·1	+ 9 30	5½ ; 7	116·4	0·94	3·16	Dob.
389	9 45·3	+27 29	8 ; 9	311·7	2·13	3·39	B.
C. 5, 8 Sextantis	9 47·5	− 7 37	5½ ; 6	85·1	0·49	4·17	B.
215, P. x. 23	10 10·9	+18 15	6½ ; 7	207·5	0·92	3·20	Dob.
424, γ Leonis	10 14·4	+20 22	2 ; 4	116·9	3·75	3·16	Ls.
218	10 22·1	+ 4 5	7 ; 9	75·5	1·09	3·29	Ls.
224	10 34·5	+ 9 22	7 ; 9	300·7	0·59	3·27	Ls.
229	10 42·3	+41 38	6 ; 7	319·3	0·88	3·40	Dob.
523, ξ Ursæ *Maj.*	11 12·9	+32 6	4 ; 5	134·2	2·34	5·51	M.

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1900+.	Observer.
		h m	° ′		°	″		
60.	Σ 1536, ι Leonis	11 18·7	+11 5	4½; 7½	53·9	2·45	3·22	Dob.
61.	OΣ 235	11 26·7	+61 38	6 ; 7½	134·5	0·50	3·41	Dob.
62.	Lalande 21846	11 24·0	+31 1	7 ; 11	5·0	1·00	4·39	Ls.
63.	Σ 1647, 191 Virginis...	12 25·5	+10 16	7½; 8	225·3	1·55	4·30	W.B.
64.	Σ 1670, γ Virginis ...	12 36·7	− 0 54	3 ; 3	328·3	5·89	3·22	Dob.
65.	Σ 1687, 35 Comæ......	12 48·1	+21 48	5 ; 8	80·5	1·04	3·47	M.
66.	Hough 257...............	13 1·2	+26 26	8½; 9	154·5	2·24	4·40	Ls.
67.	OΣ 266	13 23·6	+16 14	7 ; 8	341·8	1·63	3·33	Ls.
68.	Hough 260...............	13 19·0	+29 45	8 ; 8½	326·3	0·86	4·36	Ls.
69.	Σ 1768, 25 Can. Ven. .	13 33·2	+36 48	6 ; 8	133·4	1·11	4·42	W.B.
70.	Σ 1785	13 44·5	+27 29	7½; 8	289·2	1·45	2·57	M.
71.	β 801	13 40·7	+11 20	8 ; 10	326·5	2·63	4·46	W.B.
72.	Σ 1819	14 10·3	+ 3 36	7½; 7½	176·1	1·34	3·31	Dob.
73.	α Centauri	14 32·8	−60 25	1 ; 1	210·7	22·01	1·33	T.
74.	Σ 1865, ζ Boötis	14 36·3	+14 10	4 ; 4	151·6	0·42	4·23	B.
75.	Σ 1877, ε Boötis	14 40·6	+27 30	3 ; 6½	327·0	2·60	5·63	M.
76.	Hough 263...............	14 43·3	+24 30	7 ; 10	177·2	0·97	4·51	W.B.
77.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	176·2	2·43	5·48	M.
78.	β 31	14 47·8	+19 9	8½; 9½	193·5	1·50	4·50	W.B.
79.	OΣ 287	14 47·9	+45 20	7½; 7½	322·5	0·71	3·35	Ls.
80.	Σ 1932, ι Cor. Bor. ...	15 14·0	+27 13	6½; 7	156·8	0·70	3·71	B.
81.	Σ 1937, η Cor. Bor. ...	15 19·1	+30 40	6 ; 6	24·7	1·11	5·63	M.
82.	Σ 1938, μ^2 Boötis	15 20·7	+37 22	6½; 7½	66·7	1·03	5·60	M.
83.	OΣ 298	15 32·4	+40 8	7 ; 7½	185·5	1·17	3·35	Ls.
84.	Σ 1967, γ Cor. Bor. ...	15 38·5	+26 36	4 ; 7	116·2	0·74	3·47	Dob.
85.	OΣ 303	15 56·2	+13 33	7 ; 7½	145·3	0·91	5·52	M.
86.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	285·5	0·34	3·52	B.
87.	Σ 2032, σ Coronæ......	16 11·1	+34 8	6 ; 6½	213·5	4·58	3·43	Dob.
88.	Σ 2055, λ Ophiuchi ...	16 25·8	+ 2 12	4½; 5½	58·9	1·38	3·49	Dob.
89.	Σ 2084. ζ Herculis ...	16 37·5	+31 47	3 ; 6½	193·9	1·05	4·62	M.
90.	De 15	16 41·5	+43 40	8 ; 8½	307·6	0·68	4·66	Ls.
91.	Σ 2140, α Herculis ...	17 10·1	+14 30	2½; 6	114·7	4·62	3·46	B.
92.	Hough 415...............	17 19·4	+25 51	8 ; 8½	328·1	1·48	4·49	W.B.
93.	Σ 2173, 221 Ophiuchi.	17 25·5	− 0 59	6 ; 6	326·7	0·93	3·49	D.
94.	Σ 2262, τ Ophiuchi ...	17 57·6	− 8 11	5 ; 6	259·8	1·96	4·72	M.
95.	Σ 2272, 70 Ophiuchi .	18 0·4	+ 2 32	4 ; 6	179·1	2·02	5·55	M.
96.	OΣ 358	18 31·3	+16 54	6½; 7	192·0	1·82	3·50	W.B.
97.	β 137	18 51·2	+37 15	8 ; 8½	133·1	1·35	4·74	Ls.
98.	γ Coronæ Aust.........	19 0·0	−37 12	5½; 5½	143·3	1·93	1·40	T.
99.	Σ 2525	19 22·9	+27 8	7½; 7½	311·5	0·56	4·62	W.B.
100.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	293·2	1·61	4·89	M.
101.	Σ 2695	20 27·7	+25 28	6 ; 8	76·4	1·05	4·54	W.B.
102.	β 151, β Delphini ...	20 32·7	+14 15	3½; 5½	47·5	0·29	4·88	Ls.
103.	OΣ 413, λ Cygni	20 43·8	+36 7	5 ; 6½	64·0	0·58	4·91	Ls.
104.	Σ 2799	21 23·9	+10 38	7 ; 7½	117·0	1·37	3·56	B.
105.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	126·0	2·22	3·86	B.
106.	Σ 2878	21 50·6	+29 3	9 ; 9½	354·6	3·47	4·87	Ls.
107.	β 172	22 19·0	− 5 21	6 ; 6	7·4	0·73	2·30	D.
108.	OΣ 483, 52 Pegasi......	22 54·2	+11 12	6 ; 7½	224·7	1·05	2·90	Ls.
109.	β 79	23 12·5	− 2 4	8 ; 8½	87·0	0·97	2·92	Ls.
110.	β 80	23 13·2	+ 4 53	8 ; 8½	9·3	0·45	2·92	Ls.
111.	β 720	23 29·0	+30 46	6 ; 6	349·0	0·34	3·95	Ls.
112.	Σ 3050	23 54·6	+33 10	6 ; 6½	216·8	2·36	3·64	Dob.

In the column headed "Observer," the following abbreviations are used:—

Dob. Doberck.	M. Maw.	B. Biesbroeck.	D. Doolittle.
Ls. Lewis.	T. Tebbutt.	W.B. Bowyer.	

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

JAN 23 1906

EDITED BY
T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXIX. JANUARY, 1906. No. 366.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1905 December 8.

Mr. W. H. Maw, *President*, in the Chair.

Secretaries: E. T. Whittaker, M.A., F.R.S., and
T. Lewis.

The Minutes of the previous Meeting were read and confirmed.

Mr. Lewis announced that 61 presents had been received since the last Meeting, and specially mentioned the following of them:—W. G. Hooper, 'Æther and Gravitation' (presented by the Author); Major C. F. Close, 'Text-Book of Surveying' (presented by the Author); F. McClean, 'Photographs of the Spectrum of Nova Persei' (presented by Mrs. McClean): Photographs of the Total Eclipse of 1905 August 30, taken by the Hamburg Observatory Expedition (presented by Dr. Schorr); Copies of the Astrographic Chart of the Heavens (presented by the Royal Observatory, Greenwich); Portrait of Sir Wm. Huggins (lithograph), drawn and presented by Mr. Washington Green; and a Series of Transparencies from Negatives taken during the Total Solar Eclipse of 1905 August at Sfax, Tunis (presented by the Astronomer Royal).

On the motion of *Mr. Inwards*, seconded by *Sir W. Abney*, Mr. W. W. Bryant, Mr. F. W. Levander, and Mr. W. Shackleton were appointed Auditors of the Treasurer's Accounts for the year 1905.

The President. We have with us here to-night Prof. Schuster, who has kindly consented to give us an account of some very important researches he has recently carried out in connection with the periods of sun-spots.

Prof. A. Schuster. I must thank you, Mr. President, for this opportunity of briefly explaining to the Society some of the results which I have obtained lately by the help of a method which I think Prof. Turner has explained to you already, at a former Meeting, in reference to some magnetic investigations. In connection with this method, the optical analogy, to which he then drew attention, is so important that I should first like to say a few words which may help you to understand some of the diagrams which I shall presently project on the screen.

If a spectrum be represented diagrammatically—that is, if a curve be drawn of which the abscissæ are wave-lengths and the ordinates the intensity corresponding to particular wave-lengths—at points where there are bright lines in the spectrum the curve will shoot up to a peak, showing the length of the waves which are really periodic. Now in the case of a series of figures, we do by calculation what, in the case of light, the spectroscope does by experiment. For any series of observed variations a curve can be formed in this way. On the horizontal line, which is a time-scale, the abscissæ are marked off to represent assumed periods in days or years, and from that line ordinates are drawn showing the sum of the squares of the coefficients of the harmonic terms found by calculation to be appropriate to the different periods. I think the most perfect record of the climate of a place, or rather the most perfect distinction between the climate of one place and another, would be the temperature and rainfall plotted in curves of this kind.

One other point to complete the optical analogy. You know, from telescopic observation, that the image of a star is never an absolute point, but that it is a disk surrounded by rings. In the same way, anyone familiar with a spectroscope knows that, especially with high powers, there is never an absolute line; in a spectrum even the light is not homogeneous, but one sees a diffraction-band with bands on either side, so that one must not expect an absolute sharp rise, but a peak formed by sharp slopes. The width of the line depends on the power of the spectroscope-grating and the number of lines in the grating. Now, in the case of a series of numbers, the thing that corresponds to the power of the spectroscope is the number of periods which can be taken into consideration; that depends upon the length of the period and the length of time over which the observed material extends. When you have an 11-year period and 150 years' observations, you only have fourteen periods to work upon. Then the diffraction-band must necessarily be rather broad. One more point. It is a little puzzling when you have two lines close together in the spectrum, but quite separate. When you have only a short series of observations to investigate there may be some definite relation between two periods which cause the curve to show two small peaks close together analogous to the two lines in the spectrum. But that is a matter of detail which I just only mention to perhaps explain some of the features which may be noticed.

To apply this to the investigation of the periods of sun-spots. First of all as regards the material which I have been able to use.

We have a fairly accurate record of sun-spots since the year 1749. Sun-spots have been observed either by one observer or another ever since that time, so that Prof. Wolfer, of Zurich, has been able to collect, for every month since 1749, records which give with fair accuracy at first, and then with increasing accuracy, the number of sun-spots observed. His record is rather a combination of the number of sun-spots with a grouping of sun-spots. He gets what he calls the relative sun-spot number, which practically is the number of sun-spots observed. Since 1832 we have more accurate drawings of sun-spots which allow the measurement of areas directly, and the publication which I have used very largely is one which emanated from the Solar Physics Committee, giving the area of sun-spots for each rotation of the Sun ever since 1832. Now, of course, the two are not absolutely homogeneous in character. We have at first the sun-spot number and later the amounts of actual spot-areas, but since 1832 they overlap so that we can form estimates how these quantities can be brought into connection with each other, as I find they can be with sufficient accuracy. Whether we take the number of sun-spots or the spotted area we get the same results; there is the same difference between the sun-spots' rise and fall. I have gone particularly into that question, which is interesting for many reasons; but for our purpose to-day it may be taken that the two series are for our purpose identical and can both be used.

I think I shall best be able to explain to you what the results are by the slides which I shall use. I do not know what the practice here is, but I hope that any Fellow who wishes for any explanation will say so at once. After living with a thing for two or three years one is apt to regard some things as self-evident which are not self-evident. There is, perhaps, some question which I might answer at the beginning, and so may make the whole matter clear, for if you do not understand the first slide you will not understand the others.

This diagram is what I call a periodogram from the spectrum of the sun-spot variation, taking the whole period of time from 1749 to 1900. You see at once the big rise at 11 years. That is the 11-year period which everybody knows. There are minor variations here and there, and especially there is one which will be recognized about 5½ years, that is, the second harmonic of the 11-year period. You get the same thing here about the point corresponding to an 8-year period; you get it a little by 9·13. Now the question is, Are those little variations real or accidental? There comes in at once a difficulty. I said that the main elevation must rise to about five times the average in order to give absolutely practical certainty, but the difficulty is to know what is to be taken as the average.

Sir W. Abney. Can you explain what the diagram exactly represents?

Prof. Schuster. I can explain it by saying that this long ordinate represents the quantity obtained by working out the Fourier coefficient for a period of 11 years, and taking the square, In order to see how far these things are real and how far they may be accidental, I divided the whole time of 150 years into two parts of 75 each to see how far the results agreed, and the result was exceedingly surprising, so surprising that for a year and a half or two years I did not know what to make of it.

The next diagram shows two curves; one gives the curve for the first 75 years, the other for the last 75. There is no similarity between them at first sight; the 11-year period comes out with tremendous intensity in the last 75 years. In the first 75 years it is represented by only a little point, a point so little that I was neglecting it altogether, and it was due to that neglect that I was kept back in finding what I now believe to be the true solution.

Instead of a prominent period of about 11 years in the first 75 years, we find something like a 9½-year period, and something between 13 and 14. There is just a small peak corresponding to 11. Now in the curve showing the results for the last 75 years the dots represent what would be found if there was a truly homogeneous harmonic 11-year period; that is, in optical language, a definite line-spectrum only, and you will see how very nearly the dots agree up to a certain point here with the main result, meaning that in the last 75 years we really get a period of 11⅛ years, almost as definite as the spectrum of a sodium flame.

[*Prof. Schuster* proceeded to explain some diagrams in detail, and continued:—]

But the main conclusion I will draw to-day is this. There is a period of about 4·78 years which is represented in all the observations, the first 75 years and the last 75 years, and it is the one period which seems to continue with the same intensity—the same amplitude right through, and if we fit the curves of the first and the last periods together as regards phase, they fit almost accurately, as accurately as one could expect. Well, this is a fact which gives a very considerable amount of confidence to the result, and for a long time it seemed the only result which came definitely from this investigation, viz., that there is a period of about 4¾ years which can be traced back to about 1749, and a severe test of the reality of the period seems to me to be this—that when the maximum of this 4·78 period comes near the maximum of the ordinary sun-spot there has been an outbreak of sun-spots. That is to say, it occasionally happens that during the maximum, and also during the years of few sun-spots, there are sometimes biggish outbreaks of sun-spots, which really, I believe, in nearly all cases happens when this 4·78-year period is at a maximum. I have tried to satisfy myself as to whether that is so, and I have come definitely to the conclusion that this 4·78-year period is a real one. Had anyone asked me two months ago what the general result would have been, I would have said the result was this, the 4¾-year period certainly exists, and that as to the 11-year period it certainly seemed to have disappeared altogether during the last half of the 18th century. When I wrote out the paper in full I was very much puzzled by this fact. From the record of the last six periods I got for this 11-year period 11·125 years. Prof. Wolf, taking only the last 75, considered the maxima were separated by 11·215 instead of 11·12 years. All these periods, if you put them down in fractions, may agree with each other to one, two, or three days. When Prof. Newcomb went carefully through the sun-spot records, he rather came to the conclusion that you could trace the 11-year period back to 1600, and deduced by his method a result which is practically identical with the one I have obtained. I could not, in face of that, bring myself definitely to regard that 11-year period as only one of recent growth, and so I made further investigation.

Going back to a former diagram, it will be seen that there is indication of a period of 8·25 years. This is found from the first 75 years and has no connection with the second half; but taking this 8·25-year period, and predicting as it were, with it, it will be found that the maxima come at points when maxima are shown by the observations of the second half of the 150 years. Now that is really the thing which set me, I believe, on the right track, viz., that there are a number of periods indicated in the records of the sun-spots with practically astronomical accuracy, but that some of them occasionally seem to go to sleep and begin again after an interval, when the new curve fits the old one accurately. Taking the complete record, I find that 8·37 or 8·36 fits in a little better than 8·25 years, and this is the length I adopt provisionally.

The next slide illustrates a curious point. Between the year 1600 and the present time there have been three occasions when the interval between successive maxima was about 13·5 years: 1626 to 1639, 1816·4 to 1829·9—the interval between the two being 176 years, 13 times the period—and 1870·6 to 1883·9. The interval between the two last is 40·7 years, which is 3 times the period. I really do not attach much importance to this coincidence, for I have sufficient experience to know how often one is misled by accidental circumstances; but, taking this in connection with previous results, I think it is important. Now we have these three periods of 4·78, 8·37, and 11·125 years, and the next diagram illustrates a curious fact in connection with them. The reciprocal of 8·38 is ·11933, the reciprocal of 11·125 is ·08989. If we add these reciprocals the sum is found to be ·20922, which is the reciprocal of 4·78.

If there is a periodic cause producing an effect like sun-spots, it is not likely that this effect is proportional simply to the first power of that cause. It is likely that some higher powers will come in, but these increase more rapidly than the actual cause; then, whatever disturbs them does what in acoustics are called combination-tones do, which come in in this fashion. There comes out as a period of 35 years, a period which has often been suggested in sun-spot investigations, but there is really no definite indication. This period is too long compared with the length of the whole 150 years' record, which will only give 4 periods. We cannot possibly settle this point yet.

Now that is about as far as I got until last night. Things constantly escape one which one ought to notice. It was not till midnight last night that I found out that if you multiply 11·125 by 3 you get 33·375. If you divide that by 4 you get 8·344. I cannot be certain that the length of the period is not accurately 8·344; but this is mixed up with so many other things that again it may only be an accidental coincidence. To bring the three periods in connection it will be seen that 33·375 divided by the number of years in one period gives 4, divided by another period gives 3, and divided by the 4·8-year period gives 7. You see there is a certain connection between these periods; but how far the relation is according to some law, or accidental, I do not know. That is as far as statistics are concerned. I have tried your patience already, I am afraid, too long; and I do not know how far you may wish to go into speculative matters. I have no theory to propose to you at all, only what I think may be useful so far as it may supply some considerations to work on. There may possibly be an explanation of the very curious fact that there is an eleven years' period which comes back with perfect regularity and yet sometimes disappears from view altogether and then reappears, in that the actual sun-spot phenomena are perhaps only a secondary effect of a general disturbance in planetary space. The primary effect is what I believe is shown in corona records. If the corona be taken as a sign of disturbance in the surroundings of the Sun, that seems to me to be the primary thing, the sun-spots only coming after as secondary effect. To explain the secondary effects, the idea has been started by various astronomers who are fond of making hypotheses that the Sun acts like a cathode and projects negatively electrified particles into space; it becomes positively electrified. Well, this could not go on for ever because ultimately the Sun would be positively electrified to such an extent that the particles could not get off; they would be attracted backwards. In order that the effect should go on, that the electrons should be brought back into the Sun, we might imagine a stream of meteors which picks up in its path these electrons which have been projected; being electrified they would naturally be attracted by any solid matter, and we may see in the solar corona the effect of the discharges, and hence sun-spots. There may be besides these the electrons which are picked up as the whole solar system sweeps through space; for there may be, as far as we know, radio-active matter, and electrons distributed throughout the universe in large quantities producing large effects. All this may be picked up by streams of meteor matter; but the whole matter picked up as the whole solar system sweeps through space may depend on various accidental circumstances. It may exist in a particular part of space, so that there may be parts of the Sun's path where nothing is picked up, and therefore the sun-spot activity becomes weak. This is only an attempt to show that it is possible that there is a real cause for a period which comes quite regularly, but which may occasionally drop out from view. The relation between the different periods, of course, may be only a kind of harmonics of one big meteor-stream.

Dr. W. J. S. Lockyer. I should like to ask Prof. Schuster if he intends to continue this work, and if he will try the frequency of prominences. There are observations of prominences from the year 1870 and the number of these shows an eleven years' period; but in the curve of frequency of the prominences there seems to be another one of a much shorter period superimposed. At South Kensington it has been found to have a 3·8-year period, and we have thought there is an indication of this in sun-spots; but it was only just a slight indication, that one would never say anything definite about it. Prof. Schuster calls the period of his new sun-spot variation 4·7, and it would be of interest if he could find a 4·7-year period in the prominences.

Prof. H. H. Turner. The remark that occurs to me most prominently, Mr. President, is the striking comment that this very interesting account given us by Prof. Schuster is on the old Baconian method. If we could actually get the facts properly disposed before us, any of us could make inferences. Prof. Schuster drew my attention some time ago to this beautiful method of the periodogram; and it did seem as though we had gained a new opportunity for the Baconian method. There we had a mechanical process for exposing the material. There was the periodogram in front of us; and if there were any new facts it seemed that they would reveal themselves to us in its eccentricities. I think we have had an indication this afternoon that when we have got this periodogram the work is only beginning. It still remains for the thinker to find the clue as it did before; although it has been a little more easy for him to go to work, the work is still to come. It would have been interesting if, after what we first heard, Prof. Schuster had allowed us to meditate for a few weeks in order to see if we could find the clue which his masterly discussion has put before us. But I am afraid the opportunity has gone now, once for all. He did tell us, and we are grateful for the information, that he had worried at the thing 1½ years before he began to see daylight. I think that is an interesting fact to have on record, because now full simplicity is coming into the discussion, and we shall soon forget how difficult it was to arrive at that clue. It is very difficult for us really to discuss this material, seeing that Prof. Schuster after a couple of years' practice is only arriving at ideas night by night; and we have not yet had an opportunity of even one midnight hour to think of new suggestions. But I am sure when we get his paper we shall read it with the greatest interest; but the only difficulty I see about it is that when he gets it actually printed he will have thought of something more. I am sure we all appreciate this account immensely.

Major E. H. Hills. Has Prof. Schuster applied this method to stellar variation, or does he think there is not enough material; I mean, of course, such variable stars as are not due to revolution—a star such as Mira Ceti, whose variation may possibly be somewhat analogous to the case of the sun-spot period?

Mr. H. F. Newall. The investigation of the sun-spot problem is certainly a grand triumph for the method of Prof. Schuster—the periodogram method. It is indeed a pleasure for us that one and the same man should have given us a splendid and powerful method, and also have given us the application of it in this way. It does not often happen that one man is able to combine both points. One looks a little regretfully at all these periods at first sight, because one used to think of the 11-year period as such a nice simple thing. Now it looks almost as if the spectrum of the sun-spot period was going to be as maddening a thing as the study of the lines in the spectrum of the Sun itself. But, seriously speaking, what we have heard to-day proves to us that the periodogram method in Prof. Schuster's hands is a splendid power.

Mr. W. W. Bryant. On the curve which showed the first results of the periodogram I noticed one apex at about 9·5 to which Prof. Schuster did not refer in detail. It looked like the double of 4·78. One does not expect the double of a period to come in; but the first harmonic of 9·5, of course, would be about 4·7, so that if 9·5 were a real period, we might expect something at 4·7.

Prof. Schuster. With regard to the prominences, I should be very glad if Dr. Lockyer would take up such an investigation as he suggests; for I have my work cut out for me during the next year or two. It would be difficult to find the length of the period from the prominences. There is a very interesting relation between the two monthly solar rotation-periods and the yearly one. I have collected already a number of facts, but they are not quite complete. But I feel quite clear that there is no period of sun-spot frequency equal to the synodic solar rotation; no definitive period anywhere near it. There is one 4·7 and there is one 4·38 which comes out fairly consistently, but it is not like others that one can be certain of. These are just on the borderland, and they may be accidental and they may be real; but I am not taking them at present as real for that reason. The question of double 4·78 is puzzling: I have not mentioned that period. It does not seem to me to be actually the double of 4·78—it is nearer 9¼ years than 9½. I have had that in view, and that is why I have not said anything about 9½ years. The big rise in the curve after 9 years is to my mind no doubt due to an overlapping of two periods. One may be double of the other, and I am not able to tell you as it stands that there is not such a double period.

The calculation necessary for the research I am describing occupied an assistant working under my eyes for $1\frac{1}{2}$ years almost continuously, for it is a good long piece of work. I am very grateful to you for taking so favourable a view of the paper. I am always terribly afraid that the things one finds may be accidental and will ultimately break down; and it is only with the greatest reluctance that I come forward and suggest them as real. My first draft of the paper was really this—that there is a 4·78-year period, but as I wrote it out I could not resist putting in other things. They almost forced themselves upon me, and I hope they will justify their existence.

The President. I hope the results will be given to us in due time. We shall always be glad to see Prof. Schuster here, and to receive from him the latest results of his midnight experiences.

Mr. E. B. Knobel gave an account of his paper on the Astronomical Observations recorded in the 'Nihongi,' which it appears is a chronicle purporting to be a history of Japan from the earliest mythological times to the year A.D. 697. It contains records of several astronomical phenomena, eclipses of the Sun and Moon, occultations of stars, comets, meteors, and apparently two observations of the Aurora Borealis which happened between the years A.D. 620 and 696, and these are tabulated in the paper.

Mr. Knobel explained the elements of Japanese chronology and the origin of the symbols which stand for the names of the months—the E-to as they are called.

Mr. S. A. Saunder read a paper enumerating the difficulties in identifying formations on the lunar surface from their catalogued descriptions, because the names of the same formations differed in different catalogues, or in some cases different formations have received the same name in different catalogues; and suggested that a committee should be appointed to revise the present system of lunar nomenclature.

The President. I fully endorse what has been said about the difficulty of identifying even moderate-sized craters, to say nothing of smaller features, which, however, are of very great interest. At the same time, the difficulties attendant on a new system are also exceedingly great; and at this late hour it is impossible to discuss the various methods by which some of these difficulties might, I think, be got over. But I hope we may have an opportunity of further discussing this question, because it is really one on which, I think, those who have actually worked in examining various portions of the Moon and in getting full accounts of small details could make valuable suggestions. I do not consider it a subject to be passed over hastily at the end of a meeting. I would suggest, then, that we give an opportunity to Mr. Saunder to bring this matter forward again on a future occasion, when expressions of opinion can be gained from workers in this branch as to the methods which might be adopted in allotting names to the smaller features. Meanwhile I would propose a hearty vote of thanks to Mr. Saunder.

In the absence of Sir W. H. M. Christie, Prof. F. W. Dyson, the newly appointed Astronomer Royal for Scotland (who was received with applause), was called upon by the President to speak on the photograph of Comet 1905 *b* taken at the Royal Observatory, Greenwich.

Prof. Dyson. There is not much to say about the photograph. It was taken with the 30-inch reflector, and the exposures were very short; I think something like two minutes and a minute and a half. It was difficult to secure satisfactory photographs, because of the great velocity—about 10° a day—with which it was moving. The photograph was taken four or five days after discovery, but none have been obtained this last week owing to the weather.

Dr. Charles Chree gave the substance of his paper on "Comparison of the Results from Falmouth Declination and Horizontal Force Magnetographs on quiet Days in Years of Sun-spot Maximum and Minimum." He exhibited vector diagrams of the magnetic elements observed at Falmouth, and pointed out in what respects the curves differed in times of sun-spot maxima and minima.

Prof. Turner. May I ask whether Dr. Chree has compared his curves with the Greenwich diagrams made from observations extending over a long series of years, and, if so, what are the points of difference?

Dr. Chree. I am a little doubtful whether the correction for temperature applied in the earlier years was entirely satisfactory. Another view, suggested by Prof. Schuster's remarks this evening, is that conditions depending on the sun-spot period may have been different 30 or 40 years ago to what they are to-day. There is evidence pointing in that direction. The accordance with vector diagrams for recent years at Parc St. Maur and Kew is much closer.

The President said that the Rev. C. D. P. Davies had a mirror set up in the basement to illustrate his method of testing, and this he would be glad to show to anyone interested.

The following papers were announced and partly read:—

S. A. Saunder. "On the Present State of Lunar Nomenclature."

E. B. H. Wade. "On a New Method of Determining the Moon's Position Photographically."

H. Bourget. "Reproduction photographique des Réseaux photographiques."

Percival Lowell. "Position of the Axis of Mars."

Percival Lowell. "Comparative Charts of the Region following δ Ophiuchi."

Charles Chree. "Comparison of the Results from the Falmouth

Declination and Horizontal Force Magnetographs on quiet Days in Years of Sun-spot Maximum and Minimum."

Simon Newcomb. "Note on the Astronomical Value of Ancient Statements of Solar Eclipses."

A. W. Clayden. "On the Conditions determining the Formation of Cloud Spheres and Photospheres."

Rev. C. D. P. Davies. "On Testing Parabolic Mirrors; with some Results of the Tests as applied to some Mirrors at Oxford."

E. B. Knobel. "On the Astronomical Observations recorded in the Nihongi, the Ancient Chronicle of Japan."

P. H. Cowell. "Reply to Prof. Newcomb's Note."

P. H. Cowell. "On the Transits of Mercury, 1617–1881."

The following gentlemen were elected Fellows of the Society:—

Charles Frederic Aspinwall, B.A., Chestergate, Macclesfield.

Hubert Hayward Champion, Uppingham School, Rutland.

Lieut. Alfred Henry Laurence Ferris, R.N.R., Hughenden, Coleraine Road, Westcombe Park, S.E.

Edward MacFarlane, Under-Secretary for Lands and Chief Surveyor for New South Wales, Department of Lands, Sydney, Australia.

The following Candidates were proposed for election as Fellows of the Society:—

Robert Courtenay, B.A. (Dublin), Indian Civil Service (retired), 34 Wilmount Street, Woolwich (proposed by A. M. W. Downing).

Chetwode G. G. Crawley, A.B. (T.C.D.), Lieut.R.M.Artillery, R.N. Staff of Science Instructors H.M.S. 'Vernon,' Portsmouth (proposed by Isaac Molloy).

John Milne Gardiner Shaw, Mem. Inst. Naval Architects, F.R.Met.Soc., c/o John Swire & Sons, 8 Billiter Square, London, E.C. (proposed by E. T. Whittaker).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 November 29.

A. C. D. Crommelin, *President,* in the Chair.

Secretary: J. G. Petrie.

The Minutes of the last Ordinary Meeting were read and confirmed.

The names of 15 Candidates for Membership were read and passed for suspension, and the election by the Council of 15 new Members was confirmed.

Among the presents announced was a copy of the late Mr. F. McClean's last astronomical work (a portfolio of reproductions of photographs of the spectrum of Nova Persei), presented by Mrs. McClean, and an old 2¼-inch Gregorian reflector by Short, presented by Mr. H. R. Hanbridge.

The President said that some of these old instruments were still capable of doing very good work, and even if the one to which he was now referring was not capable of good work, it was certainly very interesting as a curiosity, to show the sort of instrument that was the best available for amateurs of moderate means at a period of nearly two centuries ago.

The President then intimated that it had been arranged to afford an opportunity to several Members of the Association who had observed the recent total eclipse to give an epitome of the reports that had been sent in, and to show a selection of slides of the photographs taken by them at the several places from which observations had been made; but before calling upon the speakers he asked Miss Stevens to explain a new kind of stand which she had designed and had had constructed by Messrs. R. & J. Beck, of 68 Cornhill, for use with small telescopes.

Miss C. O. Stevens said that the stand which was shown upon the table had been designed to meet the difficulty of finding a satisfactory mount for small telescopes and spy-glasses for the purpose of astronomical observations. The pedestal was furnished with a cup arranged to carry a heavy ball, which, when the telescope was attached to it, served by its weight to hold the instrument rigidly in any desired position without the necessity for any clamps or screws for adjustment. The telescope was thus always ready for instant use. The stand could also be used for other purposes, such as carrying a screen to receive the telescopic image of the Sun by projection—a method of solar observation that deserved to be far more generally employed than it was.

Mr. C. Thwaites, who had charge of the Association's expedition to Burgos, gave an account of the observations made by the Members and showed a large number of lantern-slides, including some from Father Cortie, Mr. Conrad W. Cooke, Miss Stevens, Mr. W. B. Gibbs, and Mr. J. H. Gear.

Col. Burton Brown, who observed the eclipse from Palma, Majorca, exhibited a lantern-slide of a composite picture of drawings by a number of observers on the west coast of the island, which was confirmed by observers on the south-east coast of Spain and others.

Capt. Alfred Carpenter, R.N., and *Mr. Keatley Moore*, members of the Burgos party, described their impressions of the eclipse.

The President said that, although Mr. Maunder obtained no astronomical results, he was sure the Meeting would be glad to hear a short account of his experiences in Labrador.

Mr. E. W. Maunder said he felt sure that the Association would sympathize with the Canadian astronomers in the great disappointment which had fallen upon them by the unfortunate weather conditions which had prevented their observation of the recent eclipse. They would do this the more heartily because all of them must greatly appreciate the extreme courtesy which had been shown to the Association by the organizers of the Canadian expedition. It was only found possible to arrange for 30 observers to take part in the expedition, but of these 30 places the Canadians placed no fewer than five at the disposal of the Association, and this although numerous applications had been received from residents in Canada who were most anxious to join the expedition. He thought that this was a most signal act of generosity; he thought, too, that the Canadian astronomers had earned their sympathy by the fulness of their preparations for the eclipse. Canada had just completed its first great official observatory at Ottawa, and this expedition was the first astronomical research that the staff of the observatory had undertaken.

A number of interesting photographs were then thrown on the screen, including portraits of Mr. Pope, the Under-Secretary of State, who had arranged for the expedition; Dr. King, the Director of the Ottawa Observatory; Mr. Plaskett, the Chief Assistant; Dr. Chant, the President of the Royal Astronomical Society of Canada; Dr. Marsh, the President of the Hamilton Astronomical Society; and other members of the expedition. A number of views of the country, the camp, and the instruments taken out were also shown.

The President said that there would only be time for three more speakers, and he would call on Messrs. Chambers, Willis, and Krauss Nield to give a brief account of their observations.

Mr. G. F. Chambers, who viewed the eclipse from the s.s. 'Arcadia' in the Mediterranean, said he was very well satisfied with the result of his experiment in observing from the deck of a steamer. The ship was perfectly steady, and there was not the slightest difficulty in observing with the telescope. The party had the advantage of 15 days' sea air and an almost calm sea. So far as their personal comfort was concerned, they were better off than those who went to Burgos.

Mr. E. C. Willis, who observed from the south of the island of Majorca, and *Mr. H. Krauss Nield*, who went to Burgos, each gave a short account of their impressions.

Mr. Dickson exhibited a photograph of the flash-spectrum that he had taken with a Thorp grating.

The President, in closing the Meeting, said that he had abstained from making any remarks, not from any want of appreciation of the different addresses, but merely to save time. He then asked the Meeting to accord a hearty vote of thanks to all who had spoken and to all others who had sent in reports, which, unfortunately, there had not been time to read at that Meeting. He hoped that at the December Meeting there would be further reports read, but he did not think it would be well to devote the whole of another Meeting to the consideration of eclipse results.

The Meeting then adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, December 20, at the Institution of Civil Engineers, Great George Street, Westminster: *Mr. Richard Bentley, F.S.A.*, President, in the Chair.

Mr. G. C. Simpson gave an interesting account of his "Attempt to fly Kites for Meterological Purposes from the Mission-ship attached to a Deep-sea Fishing Fleet in the North Sea." These observations, which were made in July and August last, were carried out on behalf of the joint Kite Committee of the Royal Meteorological Society and of the British Association. By the kindness of the Royal National Mission to Deep-Sea Fishermen, the kites were flown from the deck of the Mission-ship 'Queen Alexandra' attached to the Red Cross Fleet. Owing to the vessel being almost continuously employed in trawling, the opportunities for flying kites were very limited. Nevertheless Mr. Simpson was able to secure eight ascents during the time he was on board the vessel, and he now gave the results obtained. The greatest height reached was 5800 feet.

Mr. C. J. P. Cave described his method of flying kites in Barbados in April and May last year, and *Mr. W. H. Dines, F.R.S.*, who had examined the records, said that the humidity traces show generally a value of about 60 per cent. at the surface, rising to 80–90 per cent. at heights from 1000 to 2000 feet, and then falling off again, in some cases to 50 per cent. or less as the height increases. These values are lower than might have been expected over a tropical ocean. The increase is of the ordinary kind, but the maximum value occurs at a far lower elevation than is the case in Europe. It is probable that the relative humidity forms an extremely accurate index to the vertical circulation, a low humidity indicating a descending current of air, and so it may be inferred that there is some settling down of the atmosphere over the regions of the smaller West Indian Islands in April and May.

The other papers read at the Meeting were:—

"Temperature Observations during the Partial Solar Eclipse, August 30, 1905," by *W. H. Dines, F.R.S.*; "Comparison between Glaisher's Factors and Ferrel's Psychrometric Formula," by *J. R. Sutton, M.A.*; and "A Rapid Method of finding the Elastic Force of Aqueous Vapour, &c., from Dry and Wet Bulb Thermometer Readings," by *John Ball, Ph.D.*

The Burmese Calendar.

Burmese Astronomy is borrowed from that of the Hindus, and was introduced into Burma about 78 A.D. The several systems—

solar, lunar, sidereal, planetary—have reference to the "Myinmotaung," or Mount Meru, a legendary mountain, as a centre round which they are assumed to revolve in their different courses, from east to west, or from right to left (apparent motion). The starry firmament, enclosed within a spherical shell, is supposed to make a daily revolution about Myinmo Mount, while the Sun, Moon, and planets perform revolutions with respect to the stars, in fixed periods of time, so that their places among them can be readily ascertained.

The path of the Sun, or ecliptic, contains 12 equal divisions of 30° each called *rathis*, which correspond to the signs of the Zodiac. The Moon traverses 27 constellations (asterisms), or *nekkats*, of varying lengths. The Burmese Zodiac and Lunar Asterisms are both reckoned from the same point of the heavens, the zero of longitude (First Point of Aries).

Besides the middle asterisms, or star-clusters, referred to, there are numerous others to the north and south of the Zodiac, called *Atwintaya* and *Apyintaya*, but these have no connection with time reckonings.

There have been various Eras (*Kawza*) adopted from time to time in Burmese chronology. The original Era is supposed to have begun at midnight on Sunday, the Full Moon of Tabaung, about 9340 B.C. The Religious Era, the next but one to the present Era, in which Gaudama* is asserted to have entered into the Nirvana (Neikban) state, or migrated from this world, began in 543 B.C., the year of the Buddha's stated demise. The current or Common Era, established by King Poppasaw Yahan, commenced in 639 A.D., the first year of such Era (1) being the year 1; so that the year 1264 B.E. corresponds with 1902 A.D.†‡

The mean place of the Sun (*Madima-taik*) is computed from the Burmese Aries beginning on Saturday, 21st March§, A.D. 639, Old Style, at $24^m\ 36^s$ P.M., but the present Era commenced on the closing midnight of that day, namely, Sunday, 22nd March, Old Style (Tagu waxing 1st). A constant quantity of $\frac{386}{800}$ of a day has thus to be always added for this difference in time ($=11^h\ 35^m\ 24^s$) between the Burmese First Point of Aries and the commencement of the new Era in calculations referring the solar year to the zero of the Sun. The vernal equinox and the Burmese Aries would have coincided about 116 years earlier than the current Era. The Moon's mean place (*Madima-san*) is reckoned from a point of time $22^h\ 11^m\ 26^s$ prior to the Era, or at $1^h\ 48^m\ 34^s$ A.M. of Saturday, 21st March§, A.D. 639, Old Style; and a constant quantity of $\frac{650}{703}$ of a day has therefore to be always added owing to this autecedence in calculations referring the lunar month to the zero of new Moon.

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* Regarded as having attained Buddahood in 588 B.C.

† The Burmese eras and years, like the Christian centuries and years, are reckoned as "current" and not "expired."

‡ To obtain the Burmese year answering to the Christian year in which it begins, subtract 638 from the Christian year; thus, 1902 A.D. = 1902 − 638 = 1264 B.E.

§ Or 24th March, New Style.

The Burmese year is luni-solar, having a two-fold division—solar and lunar. The *solar* year commences when the mean Sun enters *Meiktha* (Aries), which is at present on the 15th or 16th April, and will continue so throughout the current century; in the preceding century it began on the 13th or 14th April throughout, with no exception. It consists of 365 Days 15 Nayi 31 Bizana 30 Kaya $= 365^d\ 6^h\ 12^m\ 36^s$ mean time, and is invariable. The *lunar* year is that treated of in the calendar, and contains 12 lunar months in an ordinary or common year (Matulè) and 13 in an embolismic or leap-year (Tawtulè), which occurs every second or third year, as explained further on, when an extra month (namely, the 2nd Wazo on the Burma side and the 2nd Tagu in Arakan) is intercalated. The seasons are thus kept to their proper months. In the common year there are 354 days, with months of 29 (yet masôn) and 30 (yet sôn) days alternately commencing with Tagu. A leap-year has 384 or 385 days according as it is what is known as Waugètat or Wagyitat. In the latter case an extra day (of the *yet luns*, called "yet-ngin") is added to Nayôn, while the extra month of the *la luns* called "Latat" or "Adimath" always contains 30 days. The names of the months are—Tagu, Kasôn, Nayôn, Wazo, Wagaung, Tawthalin, Thadingyut, Tazaungmôn, Nadaw, Pyatho, Tabodwè, and Tabaung.

The Burmese use the Metonic cycle of 19 years, which embraces 235 lunar months. And as 19 years of 12 months each would contain only 228 months, the extra seven months have to be distributed as equally as possible among the years. The 19 years are accordingly divided into seven parts called "adimath," each equal in length to 2 Years 8 Months $17\frac{1}{7}$ Days (= 2 Years 8 Months 17 Days 8 Nayi 2 Pat 4 Bizana 1 Pyan 7 Kaya 1 Thetha), and to each of which an extra month is allotted.

The intercalary month (Watat) usually falls in the 3rd, 5th, 8th, 11th, 13th, 16th, and 19th years, and the intercalary day (of the Wagyitat) generally in alternate periods of a cycle.

The Burmese lunar month is divided into 30 equal parts termed Didis. The mean lunar month equals $\frac{692}{703} \times 30 = 29$ Days 31 Nayi 50 Bizana $5\frac{685}{703}$ Kaya, or 29 Days 12 Hours 44 Minutes 2·3897 Seconds, nearly. A Didi thus is equal to 59 Nayi 3 Bizana $40\frac{140}{703}$ Kaya, or 23 Hours 37 Minutes 28·08 Seconds. These Didis are merely nominal divisions used for calculating the Moon's place and the date on which the year begins, and do not therefore appear in the calendar. There are two divisions of the lunar month; the one from New to Full Moon is called "Lazan," and the other from Full to New Moon, "Lazôk." The Full Moon (labyi) always takes place on the 15th of the month and the New Moon on the 14th or 15th Lazôk at the Moon's disappearance (lagwe). The mean and real (or true) New Moons seldom coincide, and the former often precedes the latter.

The Burmese days of the week are identical with those of Europe and are named, in the form of patronymics, after the Sun, the Moon, and the planets Mars, Mercury, Jupiter, Venus, and Saturn, in order. They are designated Taninganwe, Taninla, Inga, Buddahu, Kyatthabade, Thaukkya, and Sane.

The solar day is divided into 800 equal parts termed "Kyammat" for computing the Sun's longitude, and into 703 equal parts, of which 692 constitute a Didi, for computing lunar months. The Burmese solar day, astronomically, is reckoned from midnight to midnight; but the Burmese civil day began (in Burmese times) at sunrise and was composed of two divisions, the second commencing at sunset. Four natural divisions of the day were recognized, namely, sunrise (nedwet), noon (môndet), sunset (newin), and midnight (tagaung), reckoned as under:—

Day
1 o'clock before noon (Nanet titchyetti): midway between sunrise and midday.
2 „ noon (Ne hnitchyetti): midday.
3 „ afternoon (Ne thôngyetti): midway between midday and sunset.
4 „ afternoon (Ne legyetti): sunset.

Night
1 o'clock before midnight (Nya titchyetti): midway between sunset and midnight.
2 „ midnight (Nya hnitchyetti): midnight.
3 „ after midnight (Nya thôngyetti): midway between midnight and sunrise.
4 „ in the morning (Nanet legyetti): sunrise.

The Civil day was divided into 8 Baho or 60 Nayi, each Baho equalling 7½ Nayi. Obviously the Nayi varied with the length of the day or night, according to the time of the year. A gong ("Maung") was beaten every Nayi, and a drum ("Si") and a large bell ("Kaung-laung") were struck to mark every Baho. These are now, of course, things of the past, and the Europe divisions of hours are now adopted and understood practically everywhere in Burma.

The following are the Burmese * and Europe values of various years and months in mean solar days, namely :—

(*a*) Burmese.

	d	h	m	s	Days.
(i) Tropical year †	365	6	12	39	365·25875
(i) Sidereal year †	365	6	32	59·7	365·2729132
(iii) Anomalistic year †	365	6	37	34·67	365·2770951
(iv) Synodic lunar month	29	12	44	2·3898	29·530583
(v) Sidereal revolution of the Moon	27	7	43	11·2	27·3216574

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* By the "Makaranta" method.

† Tropical, sidereal, and anomalistic years are called in Burmese, Thawanamatha Hnit, Nekkattamatha Hnit, and Thuriyamatha Hnit respectively.

(*b*) Europe.

	d	h	m	s	Days.
(i) Tropical year	365	5	48	47·588	365·242217456
(ii) Sidereal year	365	6	9	10·742	365·256374332
(iii) Anomalistic year	365	6	13	45·7	365·259556701
(iv) Synodic lunar month	29	12	44	2·886368	29·5305887
(v) Sidereal revolution of the Moon	27	7	43	11·5104	27·321661

The Burmese system, though chiefly taken from the Hindus, differs from any one now in use in India, the difference being no doubt due to changes introduced from time to time by the Hindus to make their books agree with well-known facts brought to light by modern discoveries. It thus would partake more of the ancient than the modern Hindu methods. That it is essentially that of the Hindus may be gathered from the fact that the length of the solar year is the same for both countries; the 27 lunar asterisms, or lunar mansions, are identical in name, and generally have the same localities in the heavens; longitude is reckoned from the same star; the division of a lunar month into 30 equal parts is alike; the Hindu sidereal sphere is adopted by the Burmese for reckoning longitudes of the heavenly bodies; and their year consists of $365^d\ 6^h\ 12^m\ 36^s$ (=365 Days 15 Nayi 31 Bizana 30 Kaya*), which is $3^m\ 25^s$ (=8 Bizana 33 Kaya) longer than the true sidereal year—the excess, together with the precession of the equinoxes, makes an annual variation of $23^m\ 48^s{\cdot}4$ (=59 Bizana 31 Kaya) between the Burmese year and the true tropical year as computed by Europe astronomers. The Burmese reckon their longitude from the beginning of Atthawani, the first lunar asterism, *i. e.*, from the first point of the constellation Aries.

There have been in use three different methods of reckoning time and seasons, viz. (i) Thuriatheidanta, (ii) Makaranta, and (iii) Thandeikta; of these the last is that which is most commonly employed and considered the most accurate. The first is the solar system, the Súrya Siddhánta of the Hindus, adopted by King Thamôndari in 78 A.D. Next came the second or abbreviated method of King Poppasaw Yahan, who modified the rules of the Thuriatheidanta by reducing the figures into their lowest terms; this was in 639 A.D. Finally appears the modern, or demonstrative system, now generally in vogue, since 1841 A.D. (1203 B.E.) in King Pagan Min's reign, when the Burmese tropical year (mean) was put by the Nyaunggan Sadaw at 365 days 15 Nayi 31 Bizana 31 Kaya 24 Anukaya.

In respect to the Tropical and Anomalistic years, the "Adimaths" are respectively 2 Years 9 Months 0 Days 19 Nayi 24 Bizana 1 Kaya 16 Anukaya and 2 Years 9 Months 1 Day 24 Nayi 59 Bizana 50 Kaya 56 Anukaya 5 Thetha, as calculated by the "Thandeikta" method. The "Adimaths" with reference to lunations for $\frac{\text{“Didi”}}{\text{“Intha”}}$ “yets” are 2 Years $\frac{9}{8}$ Months 16 Days 3 Nayi 55 Bizana 7 Kaya 23 Anukaya. The excess of the Burmese solar year over 360 Days is 5 Days 15 Nayi 31 Bizana 31 Kaya 24 Anukaya; of the sidereal year 5 Days 16 Nayi 22 Bizana 29 Kaya 16 Anukaya; of the anomalistic year 5 Days 16 Nayi 33 Bizana 56 Kaya 41 Anukaya. A Burmese Synodic lunar month = 29 Days 31 Nayi 50 Bizana 7 Kaya; and a sidereal revolution of the Moon = 27 Days 19 Nayi 17 Bizana 58 Kaya. To bring up the solar position and have the Burmese solar year in line with the sidereal year, after allowing for precession (viz., $50''\cdot25$, which, too, is additive), a constant quantity, consisting of 1 Kaya 24 Anukaya = 1·4 Kaya = $0^s\cdot56$ = $8''\cdot40$, has to be annually added. The yearly variation is thus

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* By the "Makaranta" method.

$$50''\cdot25+8''\cdot40 = 58''\cdot65,$$

by which the Burmese celestial longitudes would annually fall short of those reckoned according to the Europe sphere, in which all such longitudes commence at the vernal equinox. The Súrya Siddhánta fixes the annual precession at 54″, or $\frac{3}{200}$ degrees. The original Súrya Siddhánta was a standard work in early times, but it was superseded by the present Súrya Siddhánta probably about 1000 A.D. The length of the year according to the two authorities was by the former put at 365 Days 15 Ghatika (Nayi) 31 Pala (Bizana) 30 Vipala (Kaya), and is by the present given as 365 Days 15 Ghatika (Nayi) 31 Pala (Bizana) 31 Vipala (Kaya) 24 Prativipala (Anukaya). J. C. Clancey.

The Astronomical Society of the Atlantic at the Cape.

The Members of the Society started for South Africa with the purpose of attending the Meeting of the British Association, but they have not yet arrived, at least in these pages. The fact is that they went a week ahead of the main party, and found so much to do in that extra week that their chronicler must beg one more indulgence from the Editors of this journal, ere he cuts the talk and comes to the—“British Asses.”

There is a peculiarity about lunch at the Royal Observatory at the Cape. It gets lost in a cloud of smoke, and suddenly it is tea-time; at any rate, that is what happens when the plans of the Senior Member from Groningen are exposed to the fierce light of practical experience that shines under one corner of Table Mountain. One of the great problems of the scheme is—How shall one get the radial velocities of the fainter stars; not of the really faint ones (that is beyond hoping for at present), but of good batches of stars down to the ninth magnitude? Can it be done by photographing a field through the objective prism, turning the prism through 180°, and repeating the exposure? If spectrum-lines can be measured like stars, one exposure might be reduced to the other in just the same way that two ordinary star-photographs are compared for parallax or proper motion. We should thus obtain the radial velocity of every star relative to the mean of all; and if there were one or two standard stars in the field whose velocities had been determined absolutely with the slit spectrograph, the thing would be done. But practical experience suggests that there would be difficulties in this elegant scheme. In the first place, there are the optical distortions of prism as well as object-glass to be eliminated. If one spreads the reduction to include distortion terms, nearly all the weight is taken away from the determination of the principal unknowns. Shall the distortions be determined, then, by an independent investigation? There are solid reasons for believing that such a course is almost always bad. The results may be interesting qualitatively, but they don't necessarily apply quantitatively to plates taken under ordinary working conditions, when the temperature changes during exposure. These difficulties are fairly obvious. There is another, more grave, that is not apt to strike those unfamiliar with the working of objective prisms. The range of measurable magnitudes on an objective prism plate is very small. On an ordinary plate the brighter stars make enlarged images, and the faintest are only grey patches; but all are more or less measurable, and a range of four magnitudes at least can be dealt with satisfactorily on one and the same plate. With an objective prism plate it is altogether different. Over-exposure very soon blots out the fine lines in the image; under-exposure gives a spectrum in which the lines themselves are equally invisible, though the spectrum itself, as a band, is conspicuous enough. The consequence is that any given exposure gives measurable images over only a very small range of star magnitude, and in the comparatively small field of a large refractor, say one degree, there are not enough stars of the right magnitude to give a weighty reduction, nor are any brighter standard stars measurable on the same plate with the fainter. It is an old difficulty very much accentuated; and apparently the scheme is unworkable with ordinary photographic refractors. But it is not clear that all these objections arise when a great portrait-lens like the Bruce telescope at Arequipa is used, though in a larger field the optical distortions of the prism, at any rate, would rapidly become big.

That his demand for velocities in the line of sight by the thousand cannot be met, owing to the existence of these instrumental difficulties, does not in the least damp the conviction of the Member from Groningen that the thing has got to be done somehow: "the investigation is necessary cosmically"; he will not be content with stars down to about the seventh magnitude, which is the working limit for spectrographic velocities at present; and if people say it can't be done—well, he is content to have aspirations higher than other people.

The "cosmical necessity" for line of sight velocities of faint stars is much more than the mere desirability of a check upon the velocity of the Sun in space derived from parallactic proper motions, even though the check-method has the very great advantage of being independent of the distance of the stars. Campbell has found the remarkable result that the fainter bright stars have larger radial velocities than the brighter. The same thing comes out even more strikingly in a determination of the solar from the parallactic motion of those two classes of stars. And in general, from stars having the greatest astronomical proper motions you get the smallest radial velocities, *which is not necessarily the consequence of their motion being mainly at right angles to the line of sight,* as one might at first suppose. It must be confessed that there seems to be something uncanny about this proposition in the theory of distribution. We had one evening an immense argument about it; discovered at the close that we had unconsciously been adopting quite different fundamental hypotheses; and never had an opportunity of returning to the subject. But as the fact, at any rate, seems to be certain, there is evidently abundant necessity for pushing the determination of radial velocities as far as instruments will go, or even rather farther. In one way or another faint stars must be got, and it is impossible to rest content with the present limits of magnitude.

Nevertheless the Member from Groningen would be very glad if people who have in their possession, and have had for years, hundreds of line of sight velocities for the brighter stars, would perceive that he has been hinting for some time that brief publication, or communication in advance of publication, would be very much appreciated by a man who has found a big new fact—the systematic "streaming" of stars—and finds himself brought to a standstill for want of data which are being held in reserve for someone else's *magnum opus.* In his paper which will be printed in full in the forthcoming B.A. report of the South African meeting, he refers sadly to the fact "that the materials for a crucial test of the whole theory by means of these radial velocities are even now on hand in the ledgers of American astronomers—alas! not yet in published form."

It has already been made clear that the Member from Groningen has a splendid faith in the power of observers and instruments to obtain the photographs which his astronomical laboratory is waiting to discuss. He wants Durchmusterung plates taken, to get parallaxes of stars down to the fourteenth magnitude. He does not think the difference of parallax between tenth magnitude and fourteenth will be more than 0″·003 or 0″·004, but is confident that plates can be taken giving a probable error of 0″·01 for each star. "That is a fact; and so with a few thousand stars it can be done. You could not get systematic error [by the photographic Durchmusterung method] if you tried. The whole matter is that we are going to find out what the Milky Way is; and as a whole the Milky Way is determined by stars of the twelfth magnitude."

It is quite evident that there is no hope of dealing with such great numbers of faint stars individually. Something like the Durchmusterung method as illustrated in the first part of the Groningen publications is the only possibility. We must take it or leave it. But the method is singularly arduous and difficult to work in a poor climate, and there is unfortunately some truth in the opinion frankly expressed at a meeting of Section A at Cape Town, that those who had first taken it were now leaving it.

All this discussion of the great scheme consumed several days, but time was left for a Society ascent of Table Mountain, and for individual pilgrimages to two historical sites. The place where Lacaille's observatory stood is in the busiest part of the town, in Strand Street, near the railway station. All traces of his house have now disappeared; but a few years ago the South African Philosophical Society placed on the wall of the building which now occupies the site—the shop of Messrs. Cooke & Sons, of York, London, and Cape Town—a very beautiful bronze tablet, with the inscription: "On this site l'Abbé de la Caille carried on his astronomical observations in the years 1751 and 1752 A.D." Above is the Southern Cross over Table Mountain and Bay; in the lower corners of the panel a quadrant and a diagram of the triangulation for Lacaille's geodetic arc*.

The famous estate of Feldhausen has been sadly spoiled during the last few years. The grounds are covered by suburban streets. The beautiful old house, an excellent piece of the old Dutch colonial architecture, with thatched roof, and a splendid tree on each side of the steps leading up to the stoep, has been altogether spoiled, roofed with corrugated iron, given a cast-iron verandah of the pattern hideously common throughout Cape Town, and turned into a surgical nursing home. The old brass sundial in the garden, its last beauty, was stolen a few months ago. The obelisk erected by Herschel's friends after his departure stands rather forlorn by the playground of an elementary school. On one side is a new-looking brass with the inscription: "Here stood from MDCCCXXXIV to MDCCCXXXVIII the reflecting telescope of Sir John F. W. Herschel, Baronet, who during a residence of four years in this colony contributed as largely by his benevolent exertions to the cause of education and humanity as by his eminent talents to the discovery of scientific truth. Erected MDCCCXLI." The astronomer may be allowed to think that this unhappy sentence in the vulgar tongue rather obscures than makes plain the circumstances which the obelisk was designed by Herschel's friends to commemorate; perhaps it accounts for the fact which Sir David Gill lamented a little while ago, that the recollection of Herschel was being lost in the very place where he worked. The otherwise admirable Cape Town handbook published for the British Association visit gives this inscription, but omits to say that on the other side of the monument there is the original brass, showing how much better some things can be put in Latin:—

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* The writer had the pleasure of bringing home a fine photograph of this memorial, as a present from the S. A. Philosophical Society to the R. A. S., which can now be seen at Burlington House.

SPECULUM SESQUIPEDALE
IN ANGLIA
PROPRIIS FACTUM MANIBUS
HOC SITU POSUIT
JOHANNES F. W. HERSCHEL
ET QUATTUOR PER ANNOS
QUOAD LONGISSIME MICANT
ORBES NEBULAEQUE COELESTES
SEDES EORUM ET SPECIES
PERITISSIME NOTAVIT.
SIC OPUS ILLUD INSIGNE
A PATRE CLARO ET A SEIPSO
BOREALI SUB COELO INCEPTUM
EODEM QUASI OCULO
IN AFRICA
FILIUS PIE PERFECIT.

The two inscriptions contain the same number of words. The Latin gets in the whole achievement of the Herschels. How curious that with this model before him the author of the inscription that is probably more often read should tell the boys that on their playground stood the telescope of a benevolent philanthropic baronet and scientific celebrity, but leave them to guess what he did with the telescope standing in his garden.

The ascent of Table Mountain by a larger (and heavier) section of the Society was all the pleasanter in that the summit was not reached. One of the members from Cambridge is an old lover of the mountain, and led us a charming way through the forests of silver trees, up over slopes covered with geranium and lobelia and gladiolus to the foot of Nursery Gorge, which gives access by a steep track to the lower plateau. There the Society lunched and slept, and instead of going the long dull stony walk to the top, went along the plateau and down by another way overlooking the vineyards of Constantia, while the Junior Member from Groningen, who had started earlier and gone another way, waited in vain upon the summit. There may be finer day's walks in the world than that up the mountain from the Newlands side, but the writer knows of none, unless perhaps it is the trip from Odde to the

Sckaeggedalsfos that was made by a few on the '96 Eclipse Expedition to Norway.

So ended the first week at the Cape, and the freedom of the Astronomical Society of the Atlantic. The Monday brought the intermediate steamer 'Durham Castle,' which had not hurried itself since we passed it off Ushant 22 days before. The Reception Room was opened. The avalanche of books and paper which greets the B. A. wherever it may meet was poured out in triple volume by the representatives of all the colonies in South Africa, and in a fashion peculiar to the country everything at the last possible moment suddenly got into order in the splendid new City Hall, ready for the arrival of the President and his Address upon the morrow. A. R. H.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Periodical Comets due in 1906.

GENTLEMEN,—

Two of the known periodical comets are due to return next year—Holmes's in the spring and Finlay's in the summer.

The former was discovered by Mr. Holmes at Islington on the 6th of November, 1892, in the constellation Andromeda, and gave a momentary suspicion of being connected with the lost comet of Biela, but its motion soon proved to be inconsistent with that hypothesis. Dr. Anderson, of Edinburgh, discovered it independently two days after Mr. Holmes. The perihelion passage had taken place, as it afterwards appeared, on the 20th of June, and it seems difficult to account for its not having been discovered earlier than it was, except on the supposition of an increase in intrinsic brightness. This comet is remarkable for the small eccentricity of its orbit, which lies wholly between those of Mars and Jupiter; the perihelion distance from the Sun is 2·2, and the aphelion distance 5·0. The period is about 6·8 years. At the next return in 1899, it was first seen by Prof. Perrine on the 11th of June, having passed its perihelion about the 28th of April. Another return to perihelion therefore will be due early next year and the comet will probably become visible in the spring.

Finlay's comet was discovered at the Cape of Good Hope on the 26th of September, 1886, and passed its perihelion on the 22nd of the following November. The period amounts to about 6½ years, and at the return in 1893 it was first seen by Mr. Finlay himself on the 17th of May, a month before perihelion passage, which was calculated to have taken place on the 16th of June. At the return in the winter of 1899 the comet was unfavourably placed and escaped observation; the next will be due in the summer of 1906. Yours faithfully,

Blackheath, 1905, Dec. 1. W. T. LYNN.

The Royal Observatory, Greenwich.

GENTLEMEN,—

I was glad to see in the *Times* of December 11 last the letter of Prof. Turner on the Royal Observatories at Greenwich and the Cape of Good Hope, called forth by a statement contained in a lately-published Blue-book, in which the work of these observatories was classed with that of other services, costing in all about £1,000,000, in such a way as to convey an impression that the observatories were expensive establishments, absorbing a large amount of money, although "mainly of scientific interest, and only indirectly of practical service to the Navy," the fact being that the annual cost of the two establishments is under £20,000.

Prof. Turner very properly points out that it was for a practical purpose that Greenwich Observatory was founded by Charles II. for the better determination of the longitude at sea (then a difficult problem causing serious delay and much misfortune, as could well be shown), and it was from observations made at Greenwich that the lunar method was made practical by the publication of the *Nautical Almanac* by Maskelyne, a succeeding Astronomer Royal, a method that was long used, although in our day practically superseded by the chronometric method, the improvement of which was greatly encouragod at Greenwich by the many trials of different constructions from the eighteenth century down to the present day; added to which Greenwich Observatory has been the store-house for the chronometers of the Royal Navy for the last 85 years, where they are tested and rated preparatory to issue to H.M. ships. The last yearly report informs us that the average daily number of chronometers under treatment during the preceding year was 686.

It was the object of Prof. Turner to show the benefit reaped by the Royal Navy from Greenwich Observatory. But what of the commercial world and the nation, from whom really come the funds for its support? The solution of the longitude problem was equally useful to the merchant marine. And the experiments made by Airy on the iron steam-ship 'Rainbow' in 1838 at Deptford, from which he deduced principles for correction of the deviation of the compass produced by the iron of a ship, which (amplified and extended) have been of benefit to the Navy, are again equally valuable to our merchant shipping, and, indeed, to the shipping of the whole world.

Again, on the establishment of the electric telegraph Greenwich Observatory was, as soon as possible, placed in telegraphic connection therewith, and since 1852 time-signals have been daily automatically transmitted through the telegraphic system for regulation of the time on railways throughout the country, a service commercially so valuable, in which our country led the way, although the application of electricity to transit-observing was first undertaken by the Americans. Another subsidiary work in which in the past Greenwich took a considerable part was the restoration of the national standards of length and weight, destroyed in the Houses of Parliament fire: a tedious work, culminating in the sending out from the Observatory of some fifty copies thereof to foreign countries, so perpetuating the British standards.

If space permitted, the public utility of Greenwich Observatory in other ways could be further illustrated.

Yours faithfully,

Blackheath, 1905, Dec. 26. WILLIAM ELLIS.

[The *Times* of December 4 contained a report of a memorandum by Lord Cawdor, then the First Lord of the Admiralty, in which the following paragraphs occurred, which led to the letter Mr. Ellis refers to.—EDS.]

> The Navy Estimates . . . . include the cost of many subsidiary services, some of which only indirectly affect the Navy, such as, for instance, Fishery Duties, Scientific Services, and the work of the Coastguard. These absorb about £1,000,000 of the money included in the Navy Estimates.
>
> The whole cost of the Observatories at Greenwich and the Cape of Good Hope falls on the Navy Estimates, although they are mainly of scientific interest, and only indirectly of practical service to the Navy.
>
> Policing the Fisheries costs £260,000 a year, which is necessarily spent on a type of ship which would not be built far war alone.
>
> It is desirable to remember how this million is spent when considering the amount of Naval Expenditure.

The Calcutta Observatory.

GENTLEMEN,—

An astronomical observatory, recently established by the Government of Bengal, is attached to the Presidency College, Calcutta, the premier Government College in India. Our astronomical equipment at present comprises two transit-instruments by T. Cooke & Sons, a few good chronometers, two astronomical clocks, a chronograph, and two equatorial telescopes, one of 7 inches aperture by Sir H. Grubb, and the other of 4½ inches aperture by T. Cooke & Sons, the latter being provided with a photographic apparatus.

Prof. A. Little, of the Presidency College, is in charge of the Observatory, and has a few Bengali assistants, who have been employed in making meteorological observations, keeping the Time Service going, systematically observing double stars in some portions of the southern hemisphere, and in observing Moon-culminations with a view to determine the longitude.

The latitude has been found by Talcott's method to be 22° 34′ 31″·2 N.

Yours faithfully,

Presidency College Observatory,
Calcutta, 1905, Nov. 30.

PHANINDRA LAL GANGOOLY.

Velocity of a Planet at any Point.

Gentlemen,—

Your December number tells me Prof. Young considers the word "ignore" in my recent letter (vol. xxviii. p. 389, line 22) "objectionable and unjust." The word was not used by me in the sense he quotes, and nothing could be farther from my intention; so I hope the Professor (to whose writings I am daily indebted) will accept my excuses and permit me now to write as a corrigendum:—

P. 389, line 22, *for* "ignore" *read* "do not mention."

I notice in same letter, line 13, "*r*" should be "D."

Yours faithfully,
Frank Robbins.

93 Aldersgate Street, E.C.,
1905, Dec. 21.

Names for Satellites.

Gentlemen,—

Allow me to thank M. Fouché for his courteous letter in the December *Observatory*. The name Triton would do very well for Neptune's satellite, if only we could arrange for its general adoption. There are still, however, left without names the newly-discovered moons of Jupiter, beginning with the fifth moon. The ninth and tenth satellites of Saturn, Phœbe and Themis, have been more fortunate.

Yours faithfully,
Chas. T. Whitmell.

Leeds, 1905, Dec. 17.

Perihelion Distance of Halley's Comet.

Gentlemen,—

In 1831 Halley's comet was within four years of a return as it is again now. The *Athenæum* for January 29 in that year printed a communication about it from Dr. J. J. G. Hartmann, of Berlin, who translated some of Pascal's works into German. In the above letter he gives some particulars respecting the comet at the returns in 1456 (which here appears as 1465, but this is evidently a misprint), 1531, 1607, 1682, and 1759. In every case the perihelion distance is stated from 11,600,000 to 11,750,000 miles. The comet, I need hardly remark, never approaches the Sun so nearly as 50,000,000 miles; so that these statements were at first puzzling. But it soon occurred to me that the communication must have been a translation and that the translator forgot that a German mile contains 4·68 English miles. A similar mistake seems to have been fallen into in the English edition of Dr. Klein's 'Star Atlas,' published in 1888, where in the letterpress we are told that the distance of 61 Cygni is about eight billions of miles (corresponding to a parallax of $0''\cdot5$). It seems worth while therefore to call the attention of translators to this. I remember when I was studying German being laughingly told by my instructor that though I might walk four English miles in an hour, I should find one German mile more than enough for that time.

To return to Halley's comet. Its perihelion and aphelion distances are, in terms of the mean distance of the Sun, 0·58 (54 millions of miles, about 13 less than that of Venus) and 35·3 (3283 millions of miles, or about 500 more than that of Neptune) respectively. He will next year be passing through the zone of small planets on his return journey. At the last return only four of these bodies were known; and though the later discoveries are nearly all of very insignificant mass, their number is so great that the comet must perforce pass very near some of them. It will probably be impossible to determine whether this produces any effect on its motion; but some such, it now seems most probable, has actually influenced by a little the period of Encke's comet.

Yours faithfully,

Blackheath, 1905, Dec. 4. W. T. Lynn.

PUBLICATIONS.

On Two Continents*.—It would be an omission for this Magazine to allow the appearance of a very interesting book recently published to pass unnoticed, though its subject is more literary than scientific. The book is interesting not only as the work of one who, in her early life, was closely connected with astronomy, but because it introduces some details bearing upon the home life of astronomers whose names have become with us household words. The object Mrs. Bayard Taylor (*née* Hansen) has before her in this book is to present to the world a fitting memorial of her husband, whose biography, whether as the translator of Faust, or subsequently as American Ambassador at the Court of Berlin, is well worthy of study. Another of Hansen's daughters was married to Wagner of Pulkova, so that these reminiscences form a connecting-link between literature, science, and politics. We are more especially interested in this lady's early life, passed as it was in the Observatory of Seeberg, for long the home of Lindenau and Encke, while with another old-world name, Baron von Zach, we are brought into close relations.

Our first picture of Hansen is one in which he is shown accompanying his sixteen-year-old wife in all the glory of her bridal attire to a public ball, and becoming lamentably unmindful of her presence. She suddenly missed her husband, and frantic with excitement ran all the way to the top of the Seeberg in her white satin slippers, to find him buried in the solution of a mathematical problem, which had occurred to him in the midst of the gay throng and had compelled him to rush headlong home to fix this idea on paper. A more pathetic scene is offered to us when later, owing to straitened circumstances, Hansen felt himself forced to quit Germany and his friends, to take up a position at Dorpat. Everything was prepared for this melancholy exile, but the necessity for the journey was fortunately prevented by an accidental meeting with the Duke of Saxe-Coburg-Gotha, to whom Hansen was able to explain the discomfort in which he found himself placed, owing to the smallness of his salary, 450 dollars. The Duke saw that these matters could be remedied, and this interview led to the removal of the Observatory from the Seeberg to Gotha. But more interesting, scientifically, is the description of a meeting between Gauss and Hansen, in which some light is thrown on the character of the former. "I once," said Hansen, "scored a point from Gauss as perhaps no one else ever did. I had been calling upon him at Göttingen, and during our talk I spoke upon the influence of refraction upon eclipses and occultations. I noticed immediately that the existence of such an influence was quite unknown to him, because he quickly changed the subject, as he was used to do whenever any topic was disagreeable to him." Hansen added that Gauss did not begin to memorize his logarithms until he found out that he (Hansen) knew them by heart.

---

* By Mrs. Bayard Taylor. Smith, Elder, & Co.

This lady accompanies her father on a visit he made to Airy, and we get not only pictures of Mrs. Airy and her daughters, but of Airy himself objecting to Hansen and Struve smoking in Greenwich Park, because Airy had a character to lose. Acknowledgment is made of Airy's kindness in providing a room in the Observatory proper where Hansen could smoke, while he corrected the proofs of his "Tables of the Sun and Moon." There seems to be some inaccuracy here about the Tables of the Sun, for Hansen's and Olufsen's Tables were published in 1853. Hansen published a supplement to these Tables, which may be the work intended here, but the tale of this room and of Airy's concession to Hansen's habit was very fresh in mind when the writer of this note went to Greenwich, but there was never any mention of the Solar Tables. One more picture of Hansen, and it shall be the last. We hear of the Duke and Duchess of Gotha, the Grand Duke and Duchess of Baden, and other royal and distinguished personages calling unexpectedly at the Observatory. Mrs. Hansen, writing to her daughter, gives the following account:—"They came unannounced at noon, while I was out, and father in dressing-gown and slippers. The Duke came first and saw father, who had recognized his voice, on the stairs, in this costume. Oh! he said, if you only put on your shoes it will be all right."

Mrs. Hansen seems to have been a charming woman, who when young learnt Latin to make the road easier for her sons to tread, and in middle life chess to amuse her husband when he was growing feeble, and, finally, mathematics, so that she could correct his proofs when her husband's eyes had grown dim with age. It does not appear that the astronomical knowledge of the distinguished translator of Faust was of a high order. Hansen was appointed President of the Commission on the Transit of Venus about 1872, but shortly after resigned, if we have put a proper construction on an expression in Taylor's letter to his wife, "Your father has given up some technical business connected with the Venus, which is a good thing." W. E. P.

NOTES.

Comet Notes.—Herr Wedemeyer gives the following parabolic elements of Comet 1905 *b* (Schaer) in *Ast. Nach.* 4062. The observations extend from Nov. 18 to Dec. 13, and are represented without very large errors:—

T	1905 Oct. 25·801 Berlin M.T.	
ω	132° 42′·7	1905·0.
☊	222 56·1	
i	140 34·9	
log *q*	0·02211	

Ephemeris for Berlin Midnight.

	R.A.	S. Dec.		R.A.	S. Dec.
	h m s	° ′		h m s	° ′
Jan. 3....	23 40 49	15 46	Jan. 19....	23 50 27	17 17
7....	23 43 5	16 16	23....	23 53 4	17 30
11....	23 45 26	16 41	27....	23 55 46	17 41
15....	23 47 54	17 0			

Messrs. Crawford and Champreux have deduced an elliptical orbit with a period of 160 years, but there is evidently not sufficient reason to abandon a parabolic orbit.

Prof. Wolf reports that this comet was visible to the naked eye in mid-November.

A comet of the eighth magnitude was discovered by M. Giacobini on Dec. 6. It is brightening rapidly, and should be a fairly conspicuous telescopic object in the evening sky in February, possibly faintly visible to the naked eye. Owing to the small perihelion-distance, the brightness will greatly increase, but when brightest the comet will be too near the Sun for observation.

The following elements were deduced by Herr Strömgren from observations extending to Dec. 16:—

T	1906 Jan. 22·63 G.M.T.
ω	198° 22′
☊	91 55
i	43 37
q	0·2238

Ephemeris for Berlin Midnight.

	R.A.	Dec.		R.A.	S. Dec.
	h m s	° ′		h m s	°
Jan. 3....	17 8 48	0 37 N.	Jan. 9....	17 55 31	6 16
5....	17 23 54	1 36 S.	11....	18 12 10	8 42
7....	17 39 28	3 53 S.	13....	18 29 33	11 13

The brightness on Jan. 13 is 18 times that at discovery.

The following ephemeris was deduced by Dr. Smart from earlier elements with a much smaller value of q; it will serve to give a general idea of the comet's behaviour after perihelion:—

	R.A.	Dec.	
	h m s	° ′	Brightness.
Jan. 25	22 8 12	20 22 S.	47
Feb. 2	23 50 16	8 4	20
6	0 33 34	1 40 S.	14
10	1 11 28	4 2 N.	10
14	1 42 48	8 48	7
18	2 9 56	12 38	4
22	2 32 56	15 40	3
26	2 52 52	18 6 N.	2

The discovery of three other comets has been announced. 1905 *d*, 1905 *e* were discovered photographically at the Lowell Observatory, Flagstaff, Arizona, on Nov. 29, but no further observations have been obtained. There is a rumour that Barnard's Periodic Comet of 1892 was detected at the La Plata Observatory, but no confirmation has been received.

A. C. D. C.

MINOR PLANET NOTES.—Planet 537 (1904 OG) has been detected on Heidelberg plates taken 1902 Feb. 24 and March 2.

A. C. D. C.

THE COMET OF 1652*.—In Van Riebeck's 'Journal of the first Hollander Settlement at the Cape,' the following notes about a comet appear:—

> Dec. 17, 1652.—This evening about between 9 and 10 o'clock saw in the E.S.E. southwards from the head of the Giant, about 80 degrees above the horizon, a strange star with a tail; the tail stretching northwards, right on to the knees of the Giant and the point most southerly about 10 degrees therefrom....
>
> Dec. 18, 1652.—Saw the star with the tail again in the same place as it was yesterday....
>
> Dec. 20, 1652.—Saw the star with the tail at present in the N.East, north of the girdle of the Giant, about 60 degrees above the horizon....

---

* Communicated by Mr. C. F. Rorke, Registrar of the Supreme Court, Pretoria.

Dec. 21, 1652.—Saw the star with the tail again in the N.E. from us, as yesterday evening....

Dec. 24, 1652.—This evening saw the star with the tail again, shot quite (heel verschoten zynde) into the N.N.W. from us, about 50 degrees above the horizon, with the tail (which is somewhat fainter than it was before) pointing to the East S. East. What it betokens God knows....

Van Riebeck's Journal was published in Holland in 1884, so I expect that if the above record is of any astronomical importance it has already been taken note of by astronomers. But it may perhaps have escaped notice, as the readers of the book are, I think, very limited in number, especially as the exact language of 1652 has been reproduced.

In any case, whether the record is interesting to astronomers or not, or whether it has already been considered by astronomers or not, I thought it would be no harm to draw attention to it. It is, possibly, one of the very earliest astronomical observations in the far south.

The Intrinsic Light of the Corona.—A paper by M. C. Fabry, published in *Comptes Rendus*, No. 23, states that the author has determined by photometric measurement that in the recent total eclipse of the Sun, at a distance of 5′ from the limb, and in the neighbourhood of the Sun's equator, the coronal light had an intrinsic value of about 720 candle-power. Comparing this with the mean intrinsic light of the Full Moon (2600 candle-power), the ratio obtained is 0·28 : 1.

Christmas Lectures at the Royal Institution.—The first of Prof. Turner's lectures on astronomy to children on December 28 was well attended, but the audience was not exclusively juvenile. It need scarcely be said that the lecture was specially explicit and entertaining, and that it was illustrated by ingenious slides and experiments, of which the following are typical. The principle of the reflecting telescope was shown by a jack-o'-lantern. The advantage of taking photographs of nebulæ was explained by a mechanical slide, which compared the size of the field of a telescope with a photograph. Another mechanical slide exhibited a comet coming into the solar system from space and travelling round the Sun, its tail growing as it moved. The dates of the other lectures are December 30, January 2, 4, 6, and 9, all at 3 P.M.

Astronomers will have heard with regret of the death, at the age of 68, of Mr. C. T. Yerkes, which took place on Friday, December 29. The story ot this rich American business man, who when it was pointed out to him that there were two large disks of optical glass in the workshops of M. Mantois of Paris awaiting a purchaser took the opportunity to buy them and then have them fashioned into the huge object-glass of the chief instrument of the observatory which bears his name, and the cost of the building of which he supplied, is well known.

THE close conjunction of Mars and Saturn (30′) on Christmas Day, which happened to be one of the very few fine nights in December, was seen by a correspondent (Mr. Maures Horner), who reports it to have been a beautiful naked-eye observation.

ATTENTION is called to the very valuable address by Prof. Schuster in the early pages of this number. Circumstances beyond control did not permit this to be submitted for the author's correction, and we therefore beg the indulgence of Prof. Schuster and of our readers for possible errors in the report.

THE next Meeting of the Royal Astronomical Society will take place on Friday, January 12; of the British Astronomical Association, on Wednesday, January 31.

FROM AN OXFORD NOTE-BOOK.

THOSE who go out on expeditions may or may not find accumulations of astronomical literature on their return. If they belong to a large observatory, the pamphlets and volumes have probably been put away by regular machinery which is not disturbed in the least by their absence; whereas in the humbler establishments the machine stops automatically when the expedition starts. Mr. Bellamy found a goodly pile of literature on his return from Egypt the other day, and in dealing with it was led into the following curious little sequence of experiences. There was a volume of meridian observations from the Observatoire d'Abbadia, of which neither the latitude nor longitude, nor any hint of the approximate locality, is given in the volume. Nor could any mention of it be found in the *Nautical Almanac* or the *Connaissance des Temps*. Ultimately an earlier volume was found giving a provisional longitude 0^{m} 16^{m} 21^{s} W. of Paris; and a latitude 43° 22′ 52″·4 N., with the appended note:—

> Nous n'avons pas d'oculaire nadiral spécial. Malgré l'attention avec laquelle on tâche de placer l'oculaire dans la même position, on ne réussit pas toujours et l'on a des effets de parallaxe atteignant 0″·5. La latitude n'est donc par (*sic* !) déterminée avec toute la rigueur désirable.

Now we are all apt to fail in seeing that a due proportion is observed between expenses of instrumental equipment and of computations or even printing; we economize in matches and are extravagant in cigars, so to speak. But is it not rather an extreme case when a few shillings or even pounds are economized for an "oculaire nadiral spécial" which would remove a source of systematic error (so it is apparently believed), while yet the money is forthcoming to make, and publish in two large volumes, meridian observations all affected by this source of error?

BUT this was not the point which particularly arrested Mr. Bellamy's attention. In looking through the *Connaissance des Temps* for 1907 to try and find the d'Abbadia Observatory, his eye was caught by the following curious entry opposite Kiel:—

	Lat.	Long.
Kappeln (église)	$54^\circ\ 39'\ 48''$	$0^h\ 30^m\ 23^s.9$
Kiel, obs. imp., 47^m	47 52 49	0 33 8·5
Idem, Sur le toit du magasin	54 20 28	0 31 14·7
de l'artill. Sig. du T. 34^m	54 19 18	0 31 17·7

It looks at first sight as though there were three Kiel observatories, one very distant from the other two, which are separated sensibly in latitude and longitude, but sadly confused in name. The explanation of the error appeared on referring to the volume for 1898 (some intermediate ones were missing), where we find:—

Kappeln (église)	54 39 48	0 30 23·9
Kaufbeuren (égl. cat) (Bav.).		
Kiel, obs. imp., 47^m	47 52 49	0 33 8·5
Idem, Sur le toit du magasin	54 20 28	0 31 14·7
de l'artill. Sig. du T. 34^m	54 19 18	0 31 17·7

Thus the position with latitude 47° odd belongs really to Kaufbeuren, but should be shifted one line up; as also the line below it. Instead of which, somebody noticed that there was no position for Kaufbeuren and so quietly dropped it out. But what a practical commentary on the way this list has been edited for at least 10 years!

It is not many years ago since Sir David Gill came back to London from Potsdam and told us of the wonderful advances made there in the determination of velocities in the line of sight. He hinted cautiously (for him) at the possibility of ultimately determining the solar parallax by this method, and I remember that one of his hearers was sceptical if not scornful, and was energetically reproved. (It is the easier to remember as it was a personal experience.) And the reproof was well deserved; for here already is Dr. Küstner presenting us with a solar parallax obtained by the method which was then only on its trial. In *Ast. Nach.* 4048–9 he produces a parallax $8''.844 \pm 0''.017$. It is true that he does not lay much stress upon this particular result as a determination of parallax, preferring rather to draw attention to the fidelity with which Doppler's principle is fulfilled. But we remember that Dr. Küstner was the first to announce definitely that there was some undoubted variation in latitudes; and we remember how quickly a whole volume of new knowledge followed his announcement; and one cannot help hoping that when he again comes forward and tells us that the time is at hand for a new step, we may again be about to reap a harvest of new results. Meanwhile his particular achievement seems to the lay mind to be of great merit.

THE *Proceedings of the Royal Society* have not only been enlarged in size and type but have been divided into two series, one for Mathematical and Physical Sciences, and the other for Biological

Sciences. The departure is not entirely new, as the *Philosophical Transactions* (which are now to be reserved for very long researches, perhaps each occupying a separate volume, similar to that on the Krakatoa Eruption) have been so divided for many years. The division of the *Proceedings* is a concession to those who desire to exclude from their libraries literature relating to sciences apart from their own. Their point of view is only too obvious. But at the same time one cannot help regretting that one of the main objects of the Royal Society, which we may take to be the unification of the Sciences, should in this way be placed second to a consideration of convenience. If a physicist does not care to read any of the biological papers, two questions are suggested: firstly, is he not unnecessarily narrow? should he not be able, for instance, to read with interest the papers on epidemics like malaria and sleeping sickness, which should appeal to every educated human being? secondly, are the bulk of the papers printed by the Royal Society of the right type if they command so little sympathy outside a narrow circle of specialists? Papers concerning one branch of science alone would surely be best placed in the proceedings of a special society: there will be no lack of others which concern two or more branches, and may even cross that line of division between physics and biology which has been emphasized by the step recently taken. It is indeed noteworthy that the line has already been crossed by one of the Secretaries. Professor Larmor contributes to Vol. 76 of Series B (Biological Sciences) a "Note on the Mechanics of the Ascent of Sap in Trees" (p. 460), and has got over an obvious difficulty of indexing by giving a cross-reference to the paper in the A series. This expedient may suffice for the present: but it will be a sad thing for both physics and biology if the gap between them is to become wider in the future.

HAVING criticized a detail in the action of the Royal Society, I now record with the greater pleasure an appreciation of its general structure which caught my eye in a new book called 'A Modern Symposium,' by G. Lowes Dickinson, viz.:—

> The Royal Society and the British Association Club are types of the right way of organizing. (p. 55.)

It seems difficult to see anything but a compliment in these words; and yet I must not suppress the fact that the words are put by the gifted author into the mouth of a man who is urging the merits of *Anarchy*.

THERE is another passage in the same work containing an elaborate metaphor from the domain of Cosmical Physics. I do not quite see the meaning of it myself, but some one else may be able to piece it together:—

> Men think the life of reason cold. How little do they know what it is to be responsive to every call, solicited by every impulse, yet still, like the magnet, vibrate ever to the north, never so tense, never so aware of the stress and strain of force as when most irremovably fixed upon that goal. The

intensity of life is not to be measured by the degree of oscillation. It is at the stillest point that the most tremendous energies meet; and such a point is the intelligence open to infinity. For such stillness I feel myself to be destined, if ever I could attain it. But others, I suppose, like MacCarthy, have a different fate. In the celestial world of souls, the hierarchy of spirits, there is need of the planet no less than of its sun. The station and gravity of the one determines the orbit of the other, and the antagonism that keeps them apart also knits them together. There is no motion of MacCarthy's but I vibrate to it; and about my immobility he revolves. But both of us, as I am inclined to think, are included in a larger system and move together on a remoter centre. And the very law of our contention, as perhaps one day we may come to see, is that of a love that by discord achieves harmony. (Pp. 75, 76.)

THE following answers to examination questions throw some new light on old dark corners of Astronomy:—

Parallax is the difference in the direction of a star in relation to other stars. The greatest difficulty urged against the theory of the earth's rotation was this. It was only when the great distance of the stars was learnt that it was thought possible that parallax might be invisible. It has now been perceived in a few stars, but only as lines drawn from a sixpence held to one eye to two miles.

Aberration the different forms which the planet take as they approach or recede from the Sun.

Parallax is the movement of a certain star in relation to other stars. Its size may be compared with the diameter of a 6*d.* and 3 miles.

Aberration; a term applied to light: it is the manner in which we receive light from the Sun. Light is sent out in rays, which spread over a large surface and give us light.

The Tides puzzle even learned astronomers, therefore a humble student can hardly hope to give a very clear and accurate account of them.

A star appears to us a point of light, and it looks bigger if it is brighter because light when it strikes the eye, causes the nerves to agitate and so reflect it to the brain again. The stronger the light the more strongly it effects the nerves. This is aberration.

The eclipse only lasts about 3 minutes, and the position of the shadow-track can never be accurately forecasted, so often astronomers who come long journeys to see one just miss it by being outside the shadow-track two or three miles.

Newton wrote the 'Principia' and *made* (*sic*!) the Universal Law of Gravitation. Refractory telescope was invented by Newton.

Bradley was a great and clever astronomer who lived during the 18th and 19th century. Some of his discoveries about the movements of the heavenly bodies are wonderful.

Newton lived in the 17th century, and was a Yorkshire man. He discovered the law of *universal* gravitation, and his chief work was his 'Principia.' He also discovered the law of inverse square.

He had made many experiments before anyone had discovered how much he knew, and when he was asked some question which other astronomers could not answer, he would say he had known the answer for some time. He discovered all the movements of the planets relatively to one another. He also invented the reflecting telescope.

FROM *Punch* for November 29:—

Are there motor-cars in the celestial regions? Professor SCHAER, of Geneva, has discovered what *he* describes as a new comet plunging due south at a rate of almost 8 degrees a day, and careering across the Milky Way regardless of all other traffic.

o. 367.] FEBRUARY, 1906. [*Price* 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, SEC.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1906 January 12.

Mr. W. H. Maw, *President*, in the Chair.

Secretaries: E. T. Whittaker, M.A., F.R.S., and
T. Lewis.

The Minutes of the previous Meeting were read and confirmed.

Mr. Lewis. Sixty-three presents have been received since the last Meeting. Attention may specially be called to the following books:—'Our Stellar Universe,' presented by the author, T. E. Heath, and the first volume of the zone of the Astrographic Catalogue, giving rectangular co-ordinates, which the Observatory of Bordeaux has in hand, presented by the French Government. The Society has also received further instalments of the enlargements of Astrographic Chart Plates: 18 from the Royal Observatory, Greenwich, 64 from the National Observatory, Paris, and 10 charts from the Observatory of San Fernando, Spain.

The President asked the Astronomer Royal to show the photograph of a comet lately taken at the Royal Observatory.

The Astronomer Royal. This photograph that I have to show is one taken with the Greenwich 30-inch reflector of Giacobini's comet 1905 *c*. It was taken on January 8th with an exposure of 12 minutes. It will be seen from the scale of degrees on the side of the plate that the tail extends to a length of more than a degree. This seems to be a very interesting comet, and it is not unlikely that it may be seen by the naked eye after it has passed perihelion, which it will do about January 22. It is now approaching the Sun, and is lost in the dawn, so that there will be very little opportunity of photographing it again just at present.

Prof. Alex. Herschel. I should like to ask if that appearance of a double tail is on the original negative.

The Astronomer Royal. There certainly seems to be a division in the tail.

The President. I think we may congratulate Greenwich on this photograph. Each month we have something from the Royal Observatory, showing that as good results can be obtained there as at observatories apparently more favourably situated.

Mr. Whittaker gave an abstract of *Prof. Barnard's* paper on "The Annular Nebula in Lyra (M 57)." In a former paper by Prof. Barnard ('Monthly Notices,' 1900 January) it was shown that measures made by Prof. Barnard in 1898 and 1899 of the central star of the nebula with reference to a star following, consistently differed from similar measures made by Prof. Burnham in 1891, and it was suggested that the nebula was in motion. Measures were subsequently made by Prof. Scheiner and Prof. Barnard of a negative taken in 1894, which did not verify this change of position. In 1902 Mr. Burt Newkirk, of Minneapolis, from a discussion of all the measures deduced a parallax and proper motion of the nebula. Since then Prof. Barnard has measured the position of the nucleus of the nebula, and these measures, which form the subject of the present paper, do not verify Mr. Newkirk's results.

Prof. Barnard referred to a 17th magnitude star, which he had mentioned in his paper of 1900 January, and stated to be at the limit of visibility of the 40-inch telescope visually. He now states that this is not so, for on 1900, July 29, with good seeing, this star could be seen very steadily.

Mr. H. F. Newall. There are few nights when this nebula can be seen in this country.

The President. The greater part of this paper consists of a tabulation of the measures, which can hardly be discussed here, so I will merely ask you to return your thanks to Prof. Barnard for his communication.

The Astronomer Royal presented a paper giving the results of the sun-spots as observed at Greenwich in the year 1904, similar in form with results communicated in previous years.

Mr. E. Walter Maunder. The sun-spot record for 1904 presented some unusual features. There was an increase in the mean daily spotted area, as compared with 1903, but this is only slight. In the two preceding cycles such a slackening in the rate of increase had meant that the actual maximum was close at hand. The mean position of the spots in latitude has also pointed in the same direction. Already in 1904 the mean latitude had come down almost to the point occupied at the maxima of the two last cycles. From these two indications it might have been concluded that the absolute maximum should have occurred early in 1905. But the actual area attained in 1904 corresponded rather to that attained nearly three years before maximum in the two last cycles, and the great increase in 1905 seems to show that we have not even yet reached the summit of the curve. The faculæ, too, showed a great increase in 1904, and were still developing. On the whole, therefore, 1904 seemed to present the somewhat abnormal case of a distinct check in the very middle of the period of increase in the solar cycle, and to point forward to the maximum being irregular and prolonged.

Prof. Turner gave an account of a paper by *Mr. H. Bourget*, of Toulouse, which had already appeared in the 'Monthly Notices' for 1905 December, on a method of making *réseaux*. To save the expense of silver-on-glass *réseaux*, which deteriorate somewhat rapidly by use, it was proposed to make a photographic copy of a silver-on-glass réseau by contact so manipulated that the printed lines should be reversed by solarization and should appear transparent on a black ground. The process is given as follows :—Take a very rapid sensitive plate, press it firmly in a printing-frame behind the réseau to be copied, expose it to the Sun for 10 seconds, develop with a quick developer. A réseau printed from a copy thus made was shown.

Prof. Turner also showed photographs of the Sun taken by Prof. Michie-Smith at the Kodaikanal Observatory on the day of the eclipse last August, showing the prominences round the Sun at that time.

Dr. W. J. S. Lockyer. I think we ought to congratulate Prof. Michie-Smith on the results he is obtaining with his spectroheliograph. In one of the *disc* pictures thrown on the screen it will be seen that a portion of a prominence has been photographed outside the limb. In the case of very bright prominences it has sometimes happened at South Kensington that the brightest portions of some prominences have been photographed with a "disc" exposure, but as a rule it takes about 60 times a "disc" exposure to obtain a good limb photograph.

The President. Were any photographs of the prominences taken at South Kensington the day before the eclipse or the day after?

Dr. Lockyer. The weather was so bad in England that there were no photographs of the limb taken for three or four days before or after. (We have, however, "disc" records for Aug. 28, 29, and 30.)*

The President. Can Dr. Lockyer tell us the differences, if any, between the South Kensington instrument and that at Kodaikanal?

Dr. Lockyer. I think Mr. Newall can answer that question better than I, as he had a great deal to do with the optical parts of the Kodaikanal instrument.

Mr. H. F. Newall. The size of the slit is the same, but the dispersing apparatus is more powerful.

Mr. Shackleton. The size of the prism at Kodaikanal is 4 inches.

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* Information communicated later.

The President. Is the difference due to climate or to instrument?

Dr. Lockyer. To both.

Mr. Maunder gave a summary of a paper by *Major Molesworth* on his observations of Jupiter. An interesting point mentioned had reference to the passage of the Great South Tropical Dark Area over the Red Spot bay. Major Molesworth observed Jupiter up to June of last year, when his observations ceased until November, but from the observed positions at these dates of the ends of the Dark Area relative to the bay, it appeared that the following end of the dark area should have been in conjunction with the preceding shoulder of the bay on August 10, and that it should have been in conjunction with the following shoulder of the bay also early in August—that is, the whole area would appear to have crossed the whole Red Spot bay (about 35° of longitude) almost simultaneously.

Major Molesworth illustrated the action by supposing a slightly curved tube, open at both ends, to represent the Belt at the Red Spot bay. The dark material may be represented by steel balls of such a size as to pass down the tube. If the tube be filled with steel balls and another be pushed into one open end, all the balls move and a ball is pushed out of the other open end.

The Rev. T. E. R. Phillips. The suggestion of Major Molesworth with reference to the manner of the passage of the dark S. tropical disturbance round the red spot is very interesting and seems quite feasible; but without further consideration of the matter I don't think I have anything more to say with regard to it.

There is one other matter relating to Jupiter, however, which I should like to refer to, if I am in order in so doing, viz., the occasional apparent flattening of the limb of the planet when a satellite is close to it. On Wednesday evening in last week I was at Bristol and was observing Jupiter in company with Mr. Denning. He was using his 10¼-inch and I his 12½-inch reflector, and shortly after the transit egress of satellite I. we both simultaneously and independently remarked on the peculiar fact that the planet's limb close to the satellite appeared quite flattened. I suppose the explanation is that the brightness of the satellite, which is greater than that of the limb of the planet, diminishes the irradiation of the latter; but why should it not always produce a similar effect? A few nights ago there were three or four transits and occultations of satellites, but though I carefully looked for a repetition of the phenomenon it was not apparent. What I did see was simply an indentation—no flattening. My attention has recently been drawn to the peculiar effect of this flattening by two or three members of the Jupiter section of the British Astronomical Association.

Mr. Newbegin. I have noticed the flattening of the limb, not on the occasion of the transit of a satellite, but in the case of occultations. One night last week I was watching the occultation of the second satellite, and as it approached the planet I noticed a distinct flattening of the limb. I followed the satellite with my eye quite up to the limb and then held it even when it seemed quite within the limb—two or three minutes perhaps after the computed time of occultation.

Rev. T. E. R. Phillips. The tabular times of the satellite phenomena given in the 'Nautical Almanac' are frequently in error by two or three minutes. No conclusion on this point can be drawn from the non-agreement of the observation with the tabular time.

The President. My own experience agrees with that of Mr. Newbegin. I have seen the apparent entrance within the limb of Jupiter at the beginning of occultations, but never at the commencement of a transit. The impression on my mind is the same as that conveyed by Mr. Newbegin, namely, that it is due to the satellite being apparently seen through the limb, the satellite overpowering the very moderate illumination of the limb of Jupiter. It would be interesting to know, of course, if the phenomenon is seen equally as well at the commencement of transit, because in that case this explanation would not hold good at all, because the satellite is on this side of the planet. I would ask if Mr. Phillips will be good enough to add to the remarks he has just made by giving us information as to the next time when the phenomenon identified by Captain Molesworth will be visible. Can he tell us when the occurrence will take place?

Rev. T. E. R. Phillips. I am afraid Jupiter will be rather too near the Sun for the next conjunction of the S. tropical disturbance with the red spot to be observed very satisfactorily. The present longitude of the red spot is just about 30°, and that of the *p.* end of the dark disturbance a little less than 80°. The latter gains about 16½° a month on the red spot, so it will be some little time yet before it quite overtakes it. This S. tropical disturbance has exhibited considerable activity of late. Last year it extended over about 45° of longitude or less, but at the present time its length is about 60°, and the activity seems to be specially marked at its *f.* end. As has been the case since the developments of the disturbance in 1901, there are white spots at its *p.* and *f.* ends, and I have noticed lately that when the *p.* spot is close to the *p.* limb it shines with an intense lustre undimmed by atmospheric absorption, almost like a satellite in transit just before egress. I have thought that this may be somewhat analogous to the fact that faculæ are readily seen near the Sun's limb though ordinarily invisible near the centre of the disc.

Mr. Lewis said that two papers on double stars had been received from the *Rev. T. E. Espin.* One paper contained measures of about 120 stars ranging in distance from 3″ to 50″. As these were pairs neglected by other observers, Mr. Espin, who is one of the few workers on double stars in England, deserves great credit. The other paper contains 51 new double stars whose separation ranges from 1½″ to 13″, most of the companions being faint. Mr. Espin observed with a 17¼-inch reflector.

Mr. Seabroke. I had, until recently, thought that it was useless to measure wide pairs, but it seems that we may derive a great deal of information from such measures. The apparent separation of two double stars seemed to have little bearing on the question as to their physical connection.

The President. It is very difficult to obtain good measures of these faint stars with refractors of the sizes ordinarily available to amateurs; but Mr. Espin teaches us that it is possible to make such measures even with a reflector, and I hope this may be an encouragement to possessors of reflectors to take up this work.

The President. Professor Turner has kindly offered to tell us something about the damage done to certain plates by transference of the grain of the wood of the plate-holders to the plates.

Professor Turner. One set of plates used during the eclipse observations at Aswan were strongly and badly marked with the woodwork of the plate-holders. It might at first be thought that it was because of the hot climate, but with the other plate-holders we got no such effect. Then anything peculiar to the makers as an explanation is also ruled out by the fact that the apparatus was made by the same makers in both cases—Messrs. Watson & Sons. We went to them before starting out to Egypt, and consulted the head of the firm as to what precautions were necessary, and I suggested that perhaps for a hot climate we ought to order metal holders. But I was advised to trust to wood, for they thought they could supply wood to satisfy all requirements. In the case of our own plate-holders these requirements were very satisfactorily met, and there were no particular marks. But the same firm made the plate-holders for Mr. Reynolds, and his were badly marked. I took his plates a few weeks ago to Dr. Russell, F.R.S., who has been working on this subject, and has already written papers upon it. He recognized the markings at once as due to the woodwork, but was not able to give me very much instruction as to how to avoid the markings. He says it is probably not radio-active action because it won't go through the glass or tin-foil. His own opinion is that it is not possible to guard against such result, however old the wood may be, and he gave me the slide now shown on the screen, in which the effect was produced by mahogany 35 years old. It is true that in this case he had to take favourable conditions to produce the effect: this plate was exposed at a temperature of 55° C., which is the best temperature, and the mahogany was put in actual contact with the plate. High temperature is favourable to the production of this effect; it can be produced by wood which has been exposed to bright sunlight, or left in the hot Sun for an hour or two. Dr. Russell showed me a series of photographs in which the wood after being subjected to the action of the Sun was then allowed to act upon the plate. Then after a couple of hours it was tried again, and after a couple of hours or so more this property died away. Well, I thought at first that this explained the reason why one set of plate-holders in Egypt produced the effect and the other did not. It is true Mr. Reynolds's carriers were more exposed to the Sun than were our own. His telescope was mounted near the railway-station at some distance away from the hotel. Our plates were kept in the hotel under cover very carefully protected from the Sun, not merely when we were going to use them, but always; whereas it was not possible to take the same precautions with Mr. Reynolds's plates. But on thinking it over I am not sure that that explanation will meet the case; because the effect was produced by one of these plate-holders in the dark room after they had not been exposed to the Sun for some days, namely after we had got them back to Cairo. So after all that Dr. Russell has told me I do not feel that we yet have got at the reason why one set of plate-holders showed this effect so badly while another in almost similar conditions did not. The point is that if we go to hot climates we must take the precautions Dr. Russell says we should—we should have plate-holders made of metal. Copper and platinum are the best, zinc and aluminium won't do, because they are also active in this way. Another way is to put the plate film inwards, because the action does not go through the glass. It is a matter which should be carefully considered, for it seems liable to happen occasionally and the conditions cannot exactly be predicted. Of course it does only happen occasionally. Until we have more certain explanation of the circumstances it seems that eclipse observers ought to take warning; and if they are really anxious to avoid this mishap, they should use metal, or put the plate-film inwards.

Prof. Turner (in answer to questions). The temperature of the dark room at Cairo was about 90° F.—not more—and it remained at that during the whole time the plates were in it, 18 hours. Of course at Aswan the temperature is much higher than that. Dr. Russell says 55° C. or 130° F. is best for this effect.

The President. A temperature of 90° F. is frequently experienced here when photographing, and it is curious that a similar effect has not been more frequently noticed.

The Astronomer Royal. Fortunately I had no such experience. But, as a matter of fact, I think our plate-holders were never in the Sun. Was it mahogany or teak or some other wood that was used in the case of Mr. Reynolds's plate-holders?

Prof. Turner. Good mahogany.

Mr. Seabroke. Could not the holders be glazed inside?

The President. Tin-foil perhaps would cure the radiation.

The Astronomer Royal. We have had considerable experience in eclipse photography—four eclipses, in which we have exposed a large number of plates—and there has never been a case of this effect. One would like to ask how long the plates were really in the plate-holders, for it would be well to know whether time enters into the question.

Prof. Turner. I cannot quite say. The plates were put in the night before, according to the usual practice at eclipses, and were carefully wrapped up in cloth and taken over to the station. The eclipse was on Wednesday—they were developed on the following Monday. Ours were developed the Friday after that.

The Astronomer Royal. Our practice is to put the plates in the night before, and have the plate-holder covered up carefully and kept in a dark place. Then to take them out as soon as possible after an eclipse; and there has never been any sign of this sort of thing—even at a temperature of 100°. Even in Sfax some new plate-holders were used, and there was no effect produced from them. So I cannot help thinking this is accidental and hardly requires such a drastic remedy as adopting metal holders, which would be heavy.

The President suggested testing plate-holders before using them to see if they did harm. He wondered whether the plate-holders had retained their power of doing harm.

Mr. Conrady. I have had opportunity of obtaining special information about the plate-holders in question. It appears that Prof. Turner's carriers were made new for this occasion—only about a month or so before the eclipse. But these carriers were from the regular stock of the firm—stock that had been in use for years. So it is really the old style that show the markings, and the new are free. I heard, however, that this occurrence is not absolutely new to the firm. They had one case in ordinary photographic practice some 17 years ago, and this is the first time since then.

Prof. Fowler. At Castellon we had a bran new camera and slides from Watsons and we had no grain-markings from the wood. The plates were put in and left for a couple of days for the purpose of testing the effect, but in spite of the high temperature no trouble was experienced. During a considerable experience of eclipse work I have never met with any difficulty of this kind.

Dr. Lockyer. I think the late Dr. Copeland during the eclipse of 1898 brought his plates home undeveloped in their slides and I think all his plates were spoilt. I think that rather adds to the evidence of time working an effect. Might I also mention that a similar action seems to be taking place with the films of kodaks. You get the films marked not only with the green-and-black paper in which the films are packed, but you get the marks on the box all produced beautifully on the film. When I returned from India in 1898 I told the Kodak Company about this. They said they would see that special precautions were taken to stop the markings; but it is just as bad today, although they have taken immense trouble about the paper with which the films are wrapped.

Prof. Turner. Dr. Copeland's experience has been always present to my mind, and I have always assumed it was because of the length of time that elapsed before he developed the slides. I consulted Mr. Kearney at Aswan as to whether it would be better to develop the plates at once or risk the journey to Cairo. He said he had had experience of photography in Egypt for a number of years, and he saw nothing against our going to Cairo. It so happens that the plates showing the markings were the early ones developed—after four days. So I return very much to what I said at the beginning. I think it is an accident which only rarely happens; but it is very disagreeable when it does happen upon an important occasion. We can, of course, adopt the President's excellent suggestion that we should take the precaution of testing our slides beforehand. Perhaps we ought to have done that before; but it is easy to be wise after the event.

The President. It would be interesting to ascertain whether the holders will mark the plates at ordinary temperature.

The Astronomer Royal. In travelling I think the precaution should be taken of removing the plates from their plate-holders.

Prof. Alex. Herschel. I might perhaps recall the fact that there was a paper relating to this subject read before the Royal Society about two years ago accompanied by numerous photographs of sections of trees taken by Dr. Russell.

The President. With regard to the markings of roller-films, that seems to happen in all cases where the sensitive surface is not protected by a sheet of gelatine, which is impervious to this particular influence. That might suggest a means of protecting the insides of these dark slides. I do not know whether Dr. Lockyer has tried that. In the Plandron films the wrapping paper has a gelatine coating on the side which comes into contact with the sensitized surface, and that seems to protect the sensitized surface from being marked by the wrapping paper.

Mr. Goodacre gave an abstract of his paper on Lunar nomenclature, in continuation of a communication made by Mr. Saunder at the last Meeting.

Mr. Goodacre said that the names adopted by Beer and Mädler had become the standard method of recognition by all subsequent selenographers, though each had made additions or alterations as he thought fit. Difficulty arose when an attempt is made to formulate a system of marking and cataloguing the minor details. The suggestion of the speaker was that the Moon should be divided into districts, each district having within its boundaries a well-known configuration to which minor objects should be referred. The largest craters in these districts should be lettered with capital Roman letters, and the 26 letters of the alphabet should suffice; and in order to make certain of this, letters should be applied only to craters above a certain diameter. The other detail of every class within these divisions should be simply numbered.

Mr. Saunder. I am glad that Mr. Goodacre has given us the benefit of his views on this subject; and I am very glad that someone as experienced as Mr. Goodacre has given us a suggestion whereby only one alphabet will suffice. That would get rid of a good deal of confusion. I do not think it will be easy to divide the Moon into counties as proposed. One practical difficulty that occurs to me is that supposing it was divided in this way, how are we to publish the divisions? Would he propose that we should re-issue Schmidt's map? that would be a serious undertaking and an expensive thing to do. Short of that I do not see any way of publishing it. What is wanted is something we can publish at moderate expense, so that anyone working at the Moon can get a copy.

Mr. Goodacre. To get over the difficulty Mr. Saunder has raised, I should propose that Schmidt's map should be reproduced photographically. At the same time a suitable *réseau* could be imprinted upon it. I do not think that would be an expensive matter. Of course to produce a new map of the Moon must be expensive.

Prof. Turner. A step has been taken in the direction of getting a Committee appointed according to Mr. Saunder's suggestion to consider this matter. The Royal Society have informed us they have sent a recommendation to the International Association of Academies urging that it should be considered by them.

The President. One of the chief difficulties in identifying lunar features, it seems to me, is due to libration. If the Moon only exposed one hemisphere to the Earth the matter would be simplified, because then there would be no difficulty in using *réseau* lines on a standard map. But the librations produce great difficulty. It seems to me that, on the whole, the simplest way of arranging the nomenclature of features of the lunar surface is to divide that surface into zones of latitude, each zone being, say, 5° wide, except near the poles, and to assign to each formation a number or letter in its own zone. It is possible for anyone with a map of the Moon to draw the lines of latitude.

The Astronomer Royal made some remarks on the meridian observations of Mösting A with the altazimuth and transit-circle at Greenwich. At Sir David Gill's suggestion meridian observations of the Lunar Crater Mösting A are being made at Greenwich and at the Cape for determination of the Moon's parallax—which is one of the primary objects for which the Cape Observatory was established. Here in this paper the results obtained at Greenwich are collected. We can only deal at present with the Right Ascensions. The observations of N.P.D are not finally reduced, for the R.—D. correction cannot be applied till later. The interest of the paper consists in this, that we have not only observations of Mösting A but of the limbs of the Moon; and with two instruments, namely the transit-circle and the new altazimuth, the observations with the latter being made in the meridian, so that the instrument is treated as a transit-circle. Altogether we have a large number of observations—about 50 of them of the crater; and the comparison between these gives some rather interesting results. The gist of the paper is really to show that the observations of the Moon's limbs are possessed of considerable accuracy and are fairly comparable with the observations of the crater, notwithstanding that different observers with different personalities are concerned in making them.

From a discussion of the discordances between the results found with the two instruments, assuming that the errors are equally divided between the two, the probable error of an observation comes out as $0^{s}{\cdot}05$. Also there are some rather curious things when we compare the observations both of the first and the second limbs with Mösting A: we find that on the whole there is a difference of $+0^{s}{\cdot}035$ between the crater and each limb with the altazimuth, but that with the transit-circle it is different. These may be partly accidental errors and partly real. There is another point referred to in the paper; and that is, the Moon's error in Right Ascension is now very considerable. It is very nearly 0·4 of a second of time for the year.

I have heard from Sir David Gill, who has sent a list of the observations made at the Cape of Mösting A; and it appears that there are only 20 days in the year on which observations have been obtained at both observatories, whereas the crater was observed on 47 days at Greenwich and 58 days at the Cape; so that it is rather unfortunate that the observations have not happened to coincide more often. I hope these observations will be continued, and that in the course of a year or two we shall get very good determinations.

Mr. Saunder. Will the Astronomer Royal kindly tell us what radius is used to compute the position of Mösting A with reference to the limbs, or is that left indeterminate, to be found from the observations?

The Astronomer Royal. The position of Mösting A is given in the 'Berliner Jahrbuch,' and we use the places of that ephemeris, but that does not in any way affect the comparison between the two observatories. I should like to add that the object of observing the crater as well as the limbs is to remove the difficulty caused by personality in observation of the limbs by different observers, the crater being nearly like a star-image.

The President. These lunar observations brought forward by the Astronomer Royal are of great interest. I am sorry that Mr. Hough, who is now in England, and was here this afternoon, has left us, for we should have been glad to hear something about what is being done at the Cape.

The following papers were announced and partly read:—

E. E. Barnard. "The Annular Nebula in Lyra."
W. E. Cooke. "Star Reductions."

W. E. Cooke. "The Work of a Colonial Observatory."

A. Stanley Williams. "Elements and Light-curve of R V Lyræ."

A. Stanley Williams. "Elements and Light-curve of V W Cygni."

L. de Ball. "The Value of the Constant of Refraction."

Royal Observatory, Greenwich. "Observations of Comet *b* 1905, from Photographs taken with the 30-inch Reflector of the Thompson Equatorial."

A. W. Roberts. "On a new Method of Determining the Absolute Dimensions of an Algol Variable."

Major P. B. Molesworth. "Report on Observations of Jupiter, 1904–5, made at Trincomali, Ceylon."

Royal Observatory, Greenwich. "Mean Areas and Heliographic Latitudes of Sun-spots in the year 1904, deduced from Photographs taken at Greenwich, at Dehra Dûn, at Kodaikánal Observatory (India), and in Mauritius."

Walter Goodacre. "Lunar Nomenclature."

Rev. T. E. Espin. "Micrometrical Measures of Double Stars: Third Series."

Rev. T. E. Espin. "New Double Stars."

H. W. Chapman. Meteor-Radiants duduced from Three Tracks."

H. C. Plummer. "Note on the Motion about an Attracting Centre of Slowly-increasing Mass."

B. T. E. Keeling. "Note on an Experiment on Lateral Refraction."

Royal Observatory, Greenwich. "Observations of Mösting A, made with the Altazimuth and Transit-Circle."

The following gentlemen were elected Fellows of the Society:—

Lieut. George Alfred Alcock, R.N.R., R.M.S. 'Athenic,' care of Shaw, Savill, & Albion Co., Royal Albert Dock, London, E.

Hugh Daniel Badcock, M.A., Assoc.M.Inst.C.E., Town Engineer, Pretoria, Transvaal, South Africa.

Rev. Arthur Edwin Brisco-Owen, M.A., The Rectory, Cholderton, Salisbury.

Wm. Robert Bruce, Observatorio Magnetico, Pilar, Provincia de Córdoba, Argentine Republic.

Lieut. Herbert Archer Edwards, R.N.R., Marine Office, Lagos, West Africa.

Crawford M. Fleming, 14 North Kinver Road, Sydenham, S.E.

William Edgar Geil, M.A., F.R.G.S., Doylestown, Pennsylvania, U.S.A., and 103 Pall Mall, S.W.

Rev. Thomas Henry Leale, A.K.C., 65 Gaskarth Road, Balham Hill, S.W.

Lieut. Frederick William Mace, R.N.R., 2 Rutland Avenue, Sefton Park, Liverpool.

William Ottway, Orion Works, Ealing, W.

Legh Richmond Powell, 10 Cranfield Road, Bexhill-on-Sea, Sussex.

Rev. John Gardham Reed, Burlington Manse, South Road, Saffron Waldon, Essex.

Richard Frind Roberts, Westcroft, Warlingham, Surrey.

The following Candidates were proposed for election as Fellows of the Society:—

Frederick Joseph William Crowe, Organist of Chichester Cathedral, Marsden, Chichester (proposed by J. A. Greenwood).

Thomas Edward Heath, 53 Park Place, Cardiff (proposed by Prof. Larmor).

Samuel Thomas Johnson, Melrose, Bridport, Dorset (proposed by Prof. H. H. Turner).

George Tyrrell MacCaw, B.Sc., Geodetic Survey, North-eastern Rhodesia, South Africa (proposed by Mr. S. S. Hough).

Percy Alfred Talbot, B.A., F.R.G.S., F.A.I., Royal Societies' Club, St. James's Street, S.W. (proposed by E. A. Reeves).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1905 December 27.

A. C. D. Crommelin, *President*, in the Chair.

Secretaries: J. G. Petrie.
J. A. Hardcastle.

The Minutes of the previous Meeting were read and confirmed.

The election by the Council of 15 new Members was unanimously confirmed and the names of four Candidates for Membership were read and passed for suspension.

The Rev. A. L. Cortie gave an abstract of the report of the Solar Section for 1902.

The President said they were greatly indebted to Father Cortie for the very interesting summary he had given of his report. It was a distinct advance in solar work not only to study the zones in latitudes where spots were most frequent and most rare, but also to look for the most active longitudes. It was a most interesting subject for investigation, whether there were regions of activity opposite one another, or whether an active longitude had a quiescent region opposite to it. Father Cortie's figures seemed to favour a quiescent region being opposite to a very active region, but it was at present a matter for further research.

Mr. E. W. Maunder said the Association had good reason to congratulate Members upon having so able a Director of the Solar Section as Father Cortie, and also upon his being able to present a record in 1902 of observations made on 350 days out of the 365. He was very glad to hear Father Cortie draw attention again to two or three important points to which he had drawn attention before, but which appeared to have passed altogether unnoticed in popular books on the Sun:—(1) The intermittent action of spot-groups; (2) The way in which one hemisphere, either north or south, carried on, as it were, the activity of the other hemisphere; (3) The way in which spots clustered into special regions of the solar surface. The intermittency of spot-action seemed to be little known. As Father Cortie was aware, it was one of the commonest things possible to see intermittent action in a Sun-spot. A group would disappear for two or three days, or it might be for a rotation or a couple of rotations, and then it would come up again in the same place. The circumstance, too, that there were on the Sun special areas which were centres of disturbance was one that could not fail to impress every careful solar observer. In his paper "Magnetic Disturbances" he had pointed out the analogy—if not the identity—between the mode of action proceeding from the Sun which gave rise to our magnetic disturbances and the coronal rays. As they all knew, the coronal rays, in the majority of instances, were not radial. If they assumed that the action taking place in the Sun was not necessarily along radial lines, it was clear they could not expect any particular meridian to be always the locus of the action which produced the spots. Indeed, if they assumed that the action took place more readily from the western limb, they would be obliged to consider that it was not radial, and would have to consider that the Earth exercised a distinct attractive influence upon that action.

Rev. A. L. Cortie said that he had not referred to the point just dealt with. What he had in his mind were statements frequently made, which attributed a special virtue to the central meridian position. To him it seemed that Sun-spots could act just as efficiently on any other meridian, supposing the mode of action to be by the ejection of streams of material particles, as on the central meridian. In fact, so far statistics showed that the west limb of the Sun was the most favourable position for a sun-spot to be connected with a magnetic storm.

Rev. A. L. Cortie then read a note on "The Aurora Borealis and Magnetic Storms of 1905, November 15."

Dr. Smart read a paper on "The Orbit of Halley's Comet."

The President said Dr. Smart's present paper would be very welcome to all interested in elementary celestial mechanics, showing the nature of the comet's motion in a very elongated ellipse. It was startling to find that, although only a small portion of the comet's orbit was outside Neptune's orbit, it took nearly half its time doing that little bit; and also that, although we are expecting the comet four years hence, it was still not very much inside the orbit of Uranus. That gives us an idea of the great pace at which it would have to move when it came near the

Sun. The President added that the Aquarid shower of meteors was identified by Mr. Denning with that accompanying this comet.

Mr. Maunder said it was not often that they had comets sufficiently bright to be within the instrumental power of the Members of the Comet Section to observe. The first comet of the year, 1905 *a*, discovered by Giacobini, was a faint telescopic comet. Within the last month there had been no fewer than five comets reported, but in the case of three of them the reports had not yet been confirmed. It was reported from the La Plata Observatory that Barnard's comet of 1892 had been discovered there. It had a period of about 6½ years, and had been searched for in 1899 without success.

The President said they were again indebted to Dr. Smart for having worked out the orbit of this comet of Giacobini, which was likely to be an interesting object at the end of January and during February.

Mr. Hardcastle communicated a paper on "The Scintillations of Stars," by *Miss M. A. Blagg*. The sky was divided into zones, and an arbitrary scale from 0 to 10 was used to indicate the amount of scintillation displayed. The final results were the product of 200, 300, or even 400 observations for each individual star, and clearly indicated that individual stars have characteristic capacities for twinkling. Capella and Procyon (both "yellow" stars) headed the list, 5·2. Betelguese followed, 4·9, and Vega 4·8. Pollux, Aldebaran, and Arcturus came next with 4·6, *a* Arietis and Altair 3·9, while *a* Cygni was lowest with 3·6. There was thus a range from 5·2 to 3·6; yellow and red stars came out high, while white stars twinkled least.

The President said that this was a somewhat original sort of paper. The twinkling of stars had, he thought, usually been treated by astronomical observers merely as a necessary evil. It would be interesting to know whether, in the observations upon which the paper was based, twinkling as regarded both motion and colour were referred to.

Mr. Hardcastle said the rapidity of change in both cases was duly noted.

Mr. E. Holmes said that Mr. Chas. Montigny, of Schaerbeck, near Brussels, had spent a good dealt of time on the subject and had invented a piece of apparatus, which he called a scintillometer.

Mr. G. M. Seabroke said that he had noticed Sirius remain a certain colour for from 3 to 5 seconds, and then change again, the changes not being sufficiently rapid to be called scintillations. He had been puzzled to account for the star remaining of one particular colour for so long. The subject might be one of considerable interest for anyone who was not engaged in doing other work.

The Rev. T. E. R. Phillips asked whether it was not a fact that stars scintillated quite as much if observed from a high altitude as from near the surface of the ground.

Mr. E. Holmes thought it was mentioned, in 'An Astronomer's Experiment,' on Teneriffe, that scintillation vanished when the observer reached a high altitude.

Mr. F. Gore said that he had watched a bright star (Fomalhaut) rising when in the Alps at an altitude of 10,000 feet, and it scintillated in a most remarkable way.

Mr. Hardcastle remarked that it might be expected of a star on the horizon even when seen from a mountain-top if the effect were atmospheric.

A paper by *Mr. Gerald H. Lepper*, on "Some Observations of Venus and Mercury during 1904 and 1905," was read by *Mr. Hardcastle.* The writer concluded that both the telescope and the spectroscope failed to solve the problem of the rotation period of Venus, which was an unsolved and, perhaps, unsolvable mystery.

The President remarked that Venus and Mercury were two very difficult objects to observe, and any evidence of any kind as regarded the nature of their markings was of great value and importance. He was only sorry Mr. Lepper had not obtained more definite results from his observations. Several slides were then shown, illustrating the recent total solar eclipse. They included photographs taken at Burgos by the Rev. T. E. R. Phillips, and others taken at Vinaros by Father Cortie. Slides of photographs also taken at Burgos by the Rev. K. Killip were also exhibited on the screen.

The Meeting adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE Annual General Meeting of this Society was held on Wednesday evening, January 17, at the Institution of Civil Engineers, Great George Street, Westminster: *Mr. Richard Bentley*, President, in the Chair.

The Council in their Report stated that the new scheme of Lectures and Exhibitions had been successfully inaugurated during the year, and that they had appointed *Mr. W. Marriott* as the Lecturer. The work of the Kite Committee had been continued, the special observations being carried out by *Mr. G. C. Simpson*, on board the Mission steamer 'Queen Alexandra,' in the North Sea. The number of Fellows is 674, being an increase of 16 on the year.

After the presentation of the Symons Gold Medal to *Sir Richard Strachey, K.C.S.I., F.R.S.*, the President, *Mr. Richard Bentley* delivered an Address on "Meteorology in Daily Life," in which he referred to the increasing interest shown lately throughout the country in the study of that science, and to the recent advances which had been made in it—more especially in the analysis of the composition of the atmosphere—and in the investigation of the upper currents of the air. He also laid stress on the increasing urgency of safeguarding the water supply, pointing out that in the reign of William the Conqueror there were barely two millions of inhabitants in these islands, and no water then used for sanitation or manufactures, and that to-day the population has risen to over forty-two millions, and most of the surface lands had either been drained or built over. In the course of his Address, Mr. Bentley also gave various illustrations of the effects of weather on human life, on the land, on navigation, on transit, &c.

The Officers and Council for the ensuing year were elected.

The Bielid Meteors in 1905.

For the second time within the last few years, disappointment has attended expectations with which a predicted reappearance of a conspicuously bright and copiously abundant star-shower was hopefully awaited. It seems to be now a pretty surely proved conclusion that in the present, as on the previous occasion of the star-shower's non-occurrence, the meteor spectacle's extinction proceeded simply in each instance from a certain marked deflecting action of planetary disturbances exerted on both meteor-streams with an exactly like tendency to abolish, in its outcome, their star-shower exhibitions. For it was shown, in 1898, by Dr. Downing, that perturbations of the 'Leonid' meteor-swarm of November 14, 1866, in the interval from 1866 to 1899, by the planets Jupiter and Saturn would have deviated the cluster's path, on its crossing the ecliptic, in November, 1899, to a distance, inwards, of nearly 1½ million miles from the Earth's orbit; and it was obviously in consequence of that deflection that no notable Leonid display was seen in that latter year, nor in the immediately earlier and following years most near the time of the shower's attaining its usual periodic maximum of brightness. In the case of the stream of Bielid meteors, it was similarly ascertained by calculations published in June last by Dr. Downing, that the dense meteor-muster of the stream encountered by the Earth on November 27th, 1885, might after three rotations, in which it would approach the planet Jupiter twice, in 1890, and more nearly in 1900–1, be expected to return on October 16th, 1905, to its descending node in the ecliptic at about 1⅛ million miles outside of the Earth's orbit in a longitude which the Earth would reach at 10^{h} P.M., G.M.T., on November 18th, last year. The Bielid shooting-stars have not, so far as I have learned, been recorded as having been noted on that night, or on those nights next following and preceding it; and in view of some considerations which here follow, it seems to be again a wholly safe and sure conjecture that the meteor-track's withdrawal to some appreciable distance from the Earth's orbit was, as in the case of the 'Leonid' stream-segment's deviated course, the real and sufficient reason, in November last, of the expected Bielid meteor-shower's display not reappearing.

Although, by the time (November 18) of the Earth's reaching the Bielid-cluster's node, that journeying segment of the belt, of 1885, would have passed the same point on October 16th, 33 days before, and would have already attained and sped a little past its perihelion, and should that swarm or segment have really marked and occupied the lost comet's place in the stream, the Earth would then be crossing the belt 33 days of current-haste along the stream (or about 100 million miles, there), behind it,—considerably in the "tail," that is to say, of the procession,—so that the shower's observed meagreness might, perhaps, on that ground be held to be partially accounted for; yet it was pointed out in 1874, by Professor H. A. Newton, that the undispersed remnants of the lost comet's head, or pair of heads, must have been quite twice as far in advance of the shower-node as the orbit's perihelion-point, when the great shower of 'Gamma-Andromedes' overspread and rained its firebrands on the Earth in November, 1872,—when, in fact, the Earth was about 200 million miles, or more than twice as far behind the real cometary head, as it was less distantly placed upon the tail in November last (and should have, therefore, incurred a still firier bombardment), from the focal group or adopted stream-head of November, 1885, then only advanced to just about as far as its orbit's perihelion.

But a glance at Mr. Denning's excellently clear and evidently very approximately truthful table (because founded on a very well-based average period of the comet's known times of revolution), in the Royal Astronomical Society's 'Monthly Notices' of last June, of the Biela's comet dates of perihelion-passage from the last year of its visibility, 1852, to the present year, interestingly compared with the last 33 years' dates of bright recurrences of the Bielid meteors, will show at once quite surely that the focal throng of meteor-dust dashed through by the Earth in November, 1885, denoted then, and must doubtless now continue still to occupy a position in the meteor-stream about 52 days of meteor-journeying onwards from the main cometary cluster, or nearly as far (between 100 and 300 million miles) in advance of it, as Professor Newton found in 1874, that the comet must have been preceded in its orbit by the notable star-showers of Bielid meteors in 1838 and 1798. We must therefore be ready to admit that the meteor-point in the comet-stream crossed by the Earth on November 18th last, 33 days behind the focal swarm of 1885, must really be also about 52–33, or 19 days ahead and in advance of the remaining parent meteor-phalanx of the lost comet's head. Taking the above latter conclusions by Professor Newton into due consideration, it can hardly be preferred as a very reasonably likely supposition, that at a point preceding the broken up comet's nuclear mass, if any vestiges are still left of it, only 19 days, or about 60 million miles, such a dearth, or barren tract of meteoritic *débris* should be found, as would be competent to explain the thorough obliteration and extinction of the Biela star-shower in the complete and total way which appears to have been indicated in November last by the continued observations made at many places. It may rather, from such evidence, be quite certainly concluded that the considerable shifting of the meteor-current's course, foretold by Dr. Downing, away from its old track across the Earth's orbit, at its descending node, has been completely well substantiated and confirmed. That a limit to this stream's dimensions and diffuseness in a direction towards the Sun, and hence also, probably in the opposite direction, no greater than about 1⅛ million miles, exists clearly in the meteor-current of the Bielids, and that it occurs also, with nearly equal narrowness in that of the Leonids, is an evidence of the extremely filamentous nature of these recently originated cometary currents, which the recent disappointments so occasioned have at least served very usefully to make clear and patent; since students of meteoric phenomena will thereby be impressed with a salutary recollection that in future similar predictions this notable restrictedness of such meteor-streams' cross-sections, already shown equally well in a transverse direction, by the Earth's quick passage through them, cannot be very wisely ignored and overlooked, or be very safely and prudently neglected.

Results of a watch kept at Slough on November 15–27, *for Leonid and Bielid meteors.*—Although on each night the Moon, ¾ full, rose at about 10^h P.M., and after that hour clouds and fog very seriously impeded observations, watches were kept at Slough for the Leonids on the nights of November 15, 16, and 17, rainy winds and fog having totally obscured the sky previously, from November 6th until the first of the latter dates. Only one meteor was seen in each of those three nights in effectively clear watches of ½ hour and 1½ hour after midnight, respectively, on the first two, and of ½ hour between 8^h and 9^h P.M. on the last of them. Neither of these three meteors was either a Lenoid or a Bielid shooting-star; but at 12^h 52^m on November 5th, an unmistakable 3rd magnitude Leonid from a radiant at 149½° +22°, or 152° +21°, had yet been observed, shooting 15° or 16° in not more than half a second, from 59° +21° to 44½° +15°. Thick hoar-frost whitened the ground, with minimum temperatures of 25° and 27°, on the nights of November 16 and 17. Watches were then kept specially for the Bielid meteors on November 18–21 and 23–27.

After a dull wintry day on November 18, the predicted date of the Earth's *rencontre* with the Bielid meteor-shower, the sky grew clear towards 8^h P.M., and in 40^m watch of the clear dark sky, before moonrise, between 8^h and 9^h 10^m, *no meteor was seen*; thick haze then overspread the sky, and although it relaxed so far that 2nd magnitude stars appeared through it for a few minutes before $9^h\ 50^m$ (when nothing meteoric was observed), it thickened again then, and from that time the sky remained quite overcast through the night.

On November 19, after a day of cloud and strong, cold north wind, the sky, though mainly overcast, was alternately clear and clouded for varying times of ½ hour to 1 hour; and in the keen wind, at 25° F., and thick hoar-frost, no persevering watch throughout the night could be maintained, in which the best opportunities which clouds and ⅝ths Moon's strong light allowed, might have been a little more fully utilized for observations. The attempt made to keep time with the sky's shifting changes by frequent in and out of door adjournments, was thus not very successful in securing intervals when the quickly closing clouds and haze unveiled the sky again for some considerable time, as happened once or twice in the later hours after midnight. But in partially clear sky for the first, and in clear dark sky for the last half-hour of a watch from 8^h to 9^h P.M., only one rather uncertain meteor (a very short- and slow-pathed Jupiter-like *o*-Cetid, apparently) was seen at $8^h\ 15^m$, so askance through thick cloud-haze low in S.E., that not much confidence was felt in its description; and in the closing clear half-hour of the sky's examination no sign at all of either Gamma-Andromedes or of any other shooting-stars was visible. In a partly clear and partly haze-impeded watch from $13^h\ 50^m$ to $14^h\ 40^m$, equal to about 25^m of effectively clear sky, with the ⅝ths Moon hiding 5th magnitude stars, no shooting-star was seen; and again in a clear watch of 8^m–10^m, at $15^h\ 25$–35^m, no appearance of a meteor was recorded.

Much clearer nights for tracing the progress of the Biela shower occurred on November 20 and 21. Ten meteors were mapped in 3 hours, till $10^h\ 35^m$, in clear, moonless sky (no certain one appearing in the first hour, 7½–8½ P.M.); and ten more in 2½ hours of rather hazed and moonlit sky, till $15^h\ 30^m$, on the first night. Of these 20 meteors, seen in 5½ hours, *only one was a pretty certain Andromede.* Two others might be counted very doubtfully in addition to the Bielid tale, but neither of them could claim to be included in it as by any means a certain member of the Bielid shower. Known radiant-points could be assigned to all the remaining noted tracks, but except three (uncertainly) from δ Cygni, and two from β Comæ, and (including one meteor from each source on the following night) three probable Polarids and two γ-Eridanids, not more than one meteor could be found to be derived from any single shower of all those scattered centres.—On the second night of November 21, three meteors were mapped in 45^m of clear, followed by 30^m of half-clouded sky, 12^h–$13^h\ 15^m$; after which hour the sky was continuously hidden through the night. One α Hydrid (of which shower a second meteor was seen on November 27), *but no γ-Andromede* was noted.—Both of these nights were sharply frosty, the lowest temperature reached being 20° F., on that of November 20. Through all the following night of November 22, clouded sky and rain, with a strong S.E. wind, and a milder minimum temperature of 40° F. prevailed.

On November 23, a perfectly clear watch of 3 hours, before moonrise, ending at 14^{h} 40^{m}, furnished 11 meteors (and two more were glimpsed), but *no γ-Andromede.* All the assigned radiant-points of these tracks, except the γ-Taurid shower's duplication, from which two more meteors proceeded on the following night, were again single-pathed, and all but four of them new to the earlier nights' shower-indications.—This calm, cool night remained clear till dawn, with minimum temperature, 30^{c} F., but the watch was not extended further, for the Biela meteors, in its later hours.

On November 24, deducting an intermission of the watch for 15^{m}, from 11^{h} 40^{m}, 15 meteors were mapped in 5 hours, in clear, moonless sky, ending at 14^{h} 25^{m}. *One neat, very foreshortened, 1–2 magnitude γ-Andromede* was recorded at 9^{h} 34^{m}, *but no other Bielid meteor*[*]. All these tracks' presumptive radiant-points, excepting the γ and ψ Taurids (with two and three meteors respectively), were single-pathed centres, and only the γ Taurid shower had (as before stated) two, and the ε and ζ Taurid radiants each one path-counterpart on November 23, and the ξ Ursids one on November 20, among the numerous non-Bielid meteor-streams more or less certainly distinguished in this and the earlier nights' watches!—Thickening white fog closed this frosty night's watch (with minimum temperature 27° F.), and a S.E. gale began, which blew with rain through all the next night of November 25. It was only in temporary lulls of this gale, which obscured the sky completely on other later nights of the month until November 30, that clear views for a few more hours of watch for the Biela meteors could be obtained on November 26 and 27.

In 25^{m} after 9^{h} 10^{m} on November 26, three meteors were mapped, a μ Perseïd (possibly a second-noted member of the shower), a ψ Taurid (a marked shower on November 24), and an ι–κ Ursid, making a third-noted meteor of that shower; while a watch of about 2½ effectively clear hours in total sum, between 8 P.M. and midnight on November 27, yielded 11 shooting-stars, but still, as on the night before, *no Bielid* meteor-track. Besides affording meteor-derivatives from five new single-pathed centres (making a total number, in all the nights, of 19 exclusively and 18 alternatively assigned centres of such showers), this night's watch contributed also three new meteors to three mapped previously from the main ε Taurid source of the active, mingled or multiple-centred radiation from Taurus in November. It also added second tracks to single ones observed before from three interesting focal centres in November, of the *p* (38) Lyncids, *p* (30) Monocerotids, and α Orionids. A. S. Herschel.

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[*] This meteor's course of 3½° in 0·8 sec., was from 26½° +48° to 24° +52°; radiant about 24° +43½°; full orange, sharply stellar head with orange tail; light-gain and loss, both even-tapered; but increased for rather most part of its course.—Of the probable Bielid meteor on November 20, the path of 6° in 0·7 or 0·8 sec., was from 26° +56° to 24° +62°; radiant about 29° +43°; mag. 4, orange-yellow, rather tailed; gain and loss of light both very even-tapered.

As the above Andromede of November 24 was the only quite unquestionable one that was observed among the 64 meteors recorded at Slough from November 18 to November 27, it is interesting to read in Mr. E. F. Sawyer's important *résumé* of his meteor observations from November 14 to November 30, in the 'Astronomical Journal' of December 22 last year (vol. xxv. p. 33), that only two (a 3rd and a 4th magnitude) Andromedes which were visible in that time at Brighton, U.S.A., were both of them seen in one hour on November 24. The radiant-point or backward intersection of the two paths was at 26° +42½°, which falls about midway between the two places, 29° +43° and 24° +43½°, given by the two above probable and certain Bielid meteor-paths observed at Slough.

[To be continued.]

Note on the Increasing Period of β Lyræ.

Observation has proved that the period of certain variable stars is not constant, but is subject to slow changes of a well-marked and regular character.

In the case of certain long-period stars—as, for instance, the southern variable R Carinæ—the cycle of errancy is completed in a definite number of years, usually measured by decades.

The period of these stars traces a regular curve, now increasing, now decreasing, and finally returning to the mean value, only to go through the cycle of changes once more.

In the case, however, of more than one close binary star we have variation of quite a different character. Here there is no certain evidence of a closed cycle of orbital changes. The period of the system is, apparently, a constantly increasing quantity—increasing, it is true at a diminishing rate, but still increasing.

The most notable instance of this type of variation in light period is to be found in the case of the well-known spectroscopic binary β Lyræ.

As far back as 1858 Argelander dealt with the phenomenon of this star's increasing period. He fully discussed the observations of early astronomers, as well as his own remarkable series of measures, and his results are given in his classical treatise 'De Stella β Lyræ variabili.'

He deduced for the changing period of β Lyræ the following values:—

Date.	Period.
1801·5	$12^d\ 21^h\ 30^m\ 15^s$
1822·7	37 2
1827·2	37 29
1838·6	42 27
1850·0	45 48

On these figures Argelander remarks:—" His numeris non solum periodum revera cum tempore variabilem esse ultra omne dubium ponitur, sed etiam quantitatem huius variationis ex observationibus meis solis inventam proxime ad veritatem accedere arguitur."

Since Argelander's day the period of β Lyræ has been frequently dealt with by astronomers.

The writer was led to consider the problem because of its kinship with many other questions arising out of the revolution of one star round another, in practical contact. Thus a definite determination of the periodic variation of Lyræ would be of material assistance in any investigation dealing with the wide subject of stellar evolution.

A discussion of all available data gave as the period of β Lyræ the varying quantity:—

$$\begin{aligned}P = {} & 12^d\ 21^h\ 59^m\ 9^s{\cdot}6 \\ & -15^s{\cdot}011\ T - 0^s{\cdot}03991\ T^2 \\ & +0^s{\cdot}001325\ T^3 - 0^s{\cdot}000014025\ T^4,\end{aligned}$$

the epoch being 1900·00.

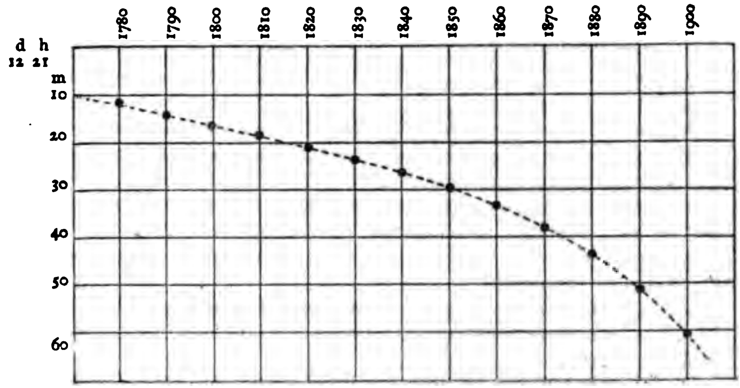
Interpreting this expression for successive dates we obtain:—

Date.	Period.			
1780	12^d	21^h	9^m	15^s
1800			26	12
1820			36	37
1840			43	30
1860			48	54
1880			54	2
1900			59	10

The diminishing rate of increase will be, probably, better exhibited in the form of a curve.

The following figure accordingly represents the period of β Lyræ from 1780 to 1900:—

Period of β Lyræ, 1780–1909.



From the table and the curve there emerge two important facts.

The rate of decrease is still diminishing: notwithstanding this diminishing rate, it is not at all probable that the variation is secular in character.

Were the variation in the light period of β Lyræ secular, then we must presuppose a vast central controlling body, and an orbit so spacious that it would take β Lyræ five centuries to traverse it. There is no need, however, to make such drafts on our imagination. A far simpler and more reasonable explanation suggests itself: an explanation, indeed, in accord with the deductions of theoretical research.

It seems reasonable to consider the progressive change in the period of β Lyræ as due to the slow recession of the component stars from one another. A very slight recession, one thousandth part of the radius of the orbit, would account for the augmentation in time, 30^{m} in a century.

Thus if we suppose the bipartition of the stars to have taken place in 1780 (this is simply a theoretical assumption), the ratio of the radius of the orbit of the system to the sum of the radii of the two components will be a ratio of unity in 1780.

This ratio in succeeding years will be:—

1800	1·00061
1820	·00099
1840	·00123
1860	·00144
1880	·00163
1900	1·00180

That the great tides which must act as a drag on the rotation-period of the close binary star β Lyræ should in a century increase the distance of the components from 1·00061 to 1·00180 is not to be wondered at. If the two orbits were drawn as large as possible on opposite pages of this journal, only a practised eye could detect the difference in size between them.

What cognate evidence can be adduced in proof of the contention that the change in the period of β Lyræ is due to the slow recession of the component stars, and not to other causes?

This, among others: that if the light-curves obtained at intervals separated by twenty or thirty years be compared—as, for example, the light-curves of Goodricke, Argelander, Schur, Markwick, and Pickering—it will be found that while their general characteristics, their family likeness, are unchanged, there are minute distinctions and pregnant points of difference, which prove that slow change is going on in the form of the light-curve, due to change in the size and form of the orbit.

Thus, recent light-curves are flatter at maxima than those found by Goodricke and Argelander. There is also a shadow of evidence that the eccentricity is increasing.

If this contention be correct, if we have in β Lyræ a binary system slowly separating under tidal forces, then we have open evidence in support of Darwin's masterly argument regarding the evolution of planetary and stellar systems.

Besides this we have in β Lyræ a dynamical problem of no ordinary interest. Usually the problem to be dealt with is the direct one: given certain conditions of equilibrium, of density, of force, what movements and changes will be the result?

In β Lyræ the problem is reversed: granted certain definite movements and changes, what are the conditions and circumstances of equilibrium, of density, of force producing them?

Alex. W. Roberts.

Notes on Jupiter.

On 1905 November 23 I witnessed transits of Satellites I. and II. and their shadows, and the time being only a few hours prior to opposition, the satellites were seen partially occulting their shadows. II. passed across Jupiter (9^h 10^m to 11^h 42^m) on the S. temperate belt and preceded the great red spot by about one hour. It was distinctly visible as a bright spot during its whole transit, cutting off the southern portion of its shadow, reduced to abnormally small proportions. At 13^h 10^m when Satellite I. was near mid-transit it formed a brightish spot on the southern side of its crescented shadow situated on the S. border of the great southern equatorial belt and immediately preceding the dusky material forming the S. tropical disturbance. Powers 315 and 440 on 12½-in. Calver reflector.

On December 8^d 11^h a small star, rather fainter than any of the four old satellites, was noticed about 50″ to N.W. of Jupiter's W. limb. With power 400 I continued making observations until 14^h 40^m, when the planet had so closely approached the star that its occultation was imminent at the northern limb of Jupiter. Clouds and showers of rain intervened, however, and prevented my actually witnessing the disappearance, which must have occurred at about 14^h 50^m.

Early in 1906 January the middle of the great red spot was in longitude 30° and its *f.* end 46°, while the middle of the S. tropical disturbance was in longitude 113° and its *p.* end 83°. As the latter marking gains 16¼° per month on the red spot, the west end of the S. tropical disturbance will come into contact with the east end of the red spot at the middle of March, while central conjunction of the two objects will occur about two months later, when Jupiter will have approached too near to the Sun for the continued study of his surface-markings.

From a great number of observations which I have consulted it appears that the first observer of the S. tropical disturbance was Major P. B. Molesworth, on 1901 February 28, so that this marking has now been visible nearly 5 years.

Observing in company with the Rev. T. E. R. Phillips on

January 3 last, we simultaneously remarked, at about 6^h 30^m, that the disc of Jupiter appeared decidedly flattened in that region where Satellite I. had, a few minutes previously, made its egress. The presence of the bright disc of the satellite so near the margin probably lessened the effect of irradiation, but it is perhaps curious the appearance is only occasionally noticed. Mr. Phillips was using the 12½-in. "Calver," while I was employing the 10-in. "With." We observed the central transit of a dark spot N. of the N.N. temp. belt at 5^h 25^m (long. 218°·4) and of a large bright rift in S. equatorial belt at 6^h 25^m (long. 54°·8).

Bishopston, Bristol,
1906, Jan. 13.

W. F. DENNING.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Spenser and the Obliquity of the Ecliptic.

GENTLEMEN,—

Mr. Hinks has called my attention to a passage at the beginning of the fifth book of the 'Faery Queene,' in which Spenser alludes to an effect produced by the diminution of the obliquity of the ecliptic, which seems to have excited the poet's fears should it continue. The passage runs thus:—

> "Ne is that same great glorious lamp of light
> That doth enlumine all these lesser fyres
> In better case, ne keepes his course more right,
> But is miscarried with the other spheres;
> For since the terme of fourteen hundred yeres,
> That learned Ptolemæe his hight did take
> He is declyned from that marke of theirs
> Nigh thirty minutes to the southerne lake,
> That makes me feare in time he will us quite forsake."

Spenser does not give his authority for this change. Danti and Tycho Brahe had in his own time determined the obliquity of the ecliptic to be something less than 23° 30′. Ptolemy's value was 23° 51′ 20″, and a comparison of these results would make the diminution about 21′·5 between A.D. 140 and 1580, or approximately 90″ in a century. (The true value is about 47″.) Kepler in the Rudolphine Tables (1627) assigns a period to the variation of 2665 years, and thinks that the obliquity reached a minimum of 23° 28′ 38″ in A.D. 1338, so that it was increasing in his own time, and would continue to do so until it reached a maximum of 23° 53′ 16″ in A.D. 2670. This theory was published in 1627, about thirty years after the death of Spenser, whose drooping spirits would have been revived had he known that the change was periodic. Kepler, however, considerably anticipated the date of minimum, and put the variation within narrower limits than we now know to be the truth, making the whole range less than half a degree, whereas its real amount is nearly three degrees.

Yours faithfully,

Blackheath, 1905, Dec. 8. W. T. LYNN.

The Herschel Obelisk at the Cape.

GENTLEMEN,—

The notice, in your January issue, of the two inscriptions on the Herschel Obelisk at the Cape of Good Hope was written evidently under a misapprehension about the facts, which I feel sure the writer will allow me to correct, since they lay beyond his knowledge.

The English, and not the Latin, inscription is the "original" one, for it was composed, perhaps by Sir Thos. (then Mr.) Maclear, in 1839, or thereabouts, and received the approval of Lady Herschel; and has since then been lying among the archives of the Cape Observatory. ·

It has now been engraved and placed on the Obelisk by Admiral Maclear, in fulfilment of his father's ardent desire to see the monument completed; another case of "filius pie perfecît." It commemorates, in language necessarily general, the social as well as the scientific labours of Sir John in Africa.

The other inscription was composed last year in Latin, because it is intended for lovers of science who can appreciate the astronomical value of the spot on the Earth's surface.

Both inscriptions were fixed where they now are in 1905.

92 Woodstock Road, Oxford. Yours faithfully,
1906, Jan. 17. W. J. HERSCHEL.

[I must thank Sir William Herschel for his correction with regard to the English inscription on the Herschel Obelisk at the Cape. It was, as I remarked, newly erected (1905); that it had been written so long ago as 1839 was beyond my knowledge.

But I confess to having some difficulty in understanding how Sir William can be correct in saying that the Latin inscription was both composed and erected in 1905.

The English brass preserved most of its lacquer, the paint was still in the letters, and the whole thing looked new. The Latin brass was black and corroded, not very easily legible, and with every appearance of age, when I saw it on 1905, Sept. 19. To the best of my recollection, one brass is on the east, the other on the west face of the monument, so that weathering would be equal, and it is not easy to believe that the Latin brass was at most eight months old when I copied the inscription. Is it possible that Sir William Herschel may himself be under some misapprehension with regard to the history of the Latin inscription?—A. R. HINKS.]

The Solar Corona as observed in 1715.

GENTLEMEN,—

The association between types of the corona as seen in total eclipses of the Sun and the state of the periodicity of solar spots is now probably one of the best-established facts which have resulted from the systematic observations of eclipses made now during more than sixty years. For the earliest drawings, however, made of the corona we must go back to the year 1715, when the total eclipse of April 22 O.S. (May 3 * N.S.) was visible as such in England, and was observed, amongst others, by Roger Cotes, the first Plumian Professor of Astronomy at Cambridge, and of whom, on his premature death in the following year, Newton remarked that "had he lived, we might have known something." The drawing made by Cotes was not published until 1850 in the 'Correspondence of Sir Isaac Newton and Prof. Cotes,' edited by J. Edleston. With regard to this the learned author of the life of Cotes, in the 'Dictionary of National Biography,' states that it resembles the types of corona seen in 1867 and 1878, when sun-spots were at a minimum, and as 1715 was at or near an epoch of sun-spot maximum and the spots were numerous, this "goes far to disprove the supposed connexion." Now, in examining this point, it should be remembered that Halley (who observed the eclipse at Crane Court, London) tells us that Cotes "had the misfortune to be oppressed by too much company" at the time, so that he only saw the totality for part of the short time that it was visible. His drawing looks more like a sort of diagram by a person not accustomed to artistic representation. But, in addition, he gives us, besides his own figure, a view of a drawing made by "a very ingenious gentleman" (presumably at Cambridge also, but he does not mention his name), and this has much less resemblance to the type of corona usually seen at a minimum epoch of solar spots; in fact the corona wholly surrounds the Sun, and the polar prolongation does not seem fainter than the equatorial, as Cotes thought it. Both drawings are reproduced in Ranyard's Eclipse volume (xli.) of the 'Memoirs of the Royal Astronomical Society'; we are told (p. 501) that the originals had been inquired for at the Library of Trinity College, but had not been found.

Yours faithfully,

Blackheath, 1906, Jan. 1. W. T. LYNN.

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* In the Eclipse volume of the 'Memoirs of the R. A. S.' the date is given May 2nd, but it should be 3rd. Oddly enough, Cotes's letter to Newton is there also dated May 2nd, which is really correct by O.S., corresponding to May 13 N.S., by which Edleston dates it.

PUBLICATIONS.

Star Stereograms*.—On page 258 of the last volume of this Magazine a short account was given of an ingenious device arranged by Mr. T. E. Heath, of Cardiff, for showing the Universe of the brighter stars as it would appear to an observer transported far away into space in various directions. This stereoscopic arrangement was exhibited at the Royal Society on two occasions, and for two months in the Library of the Royal Astronomical Society, and met with some approval. Mr. Heath has now produced an analogous work. This time he looks stereoscopically at the Universe from the Earth. His 26 small star-charts which cover the whole sky—8 whose centres are in declination 45° N., 8 whose centres are in declination 45° S., 8 which have their centres on the equator, and 1 each for the N. and S. poles—are made stereoscopically. The author has collected the measured parallaxes of those stars for which it was available, and has divided the remainder into two types according to their spectra, and has assigned an average parallax to each type. Then multiplying the parallax by a large quantity, 19,000, so as to exaggerate the distribution in three dimensions, and so make it apparent to the eye, he has constructed these stereograms. The result is most successful. Putting chart number 12 into a stereoscope, one sees Sirius and Procyon apparently hanging in space well in front of all the other stars, β Orionis well behind α Orionis, and the background of smaller stars at varying distances. It cannot be said that they appear actually at their relative distances apart in all cases, but it would have been difficult to do this exactly. The book contains 26 key-maps, which show the same stars as the stereograms, with their magnitudes, spectral type, and parallax (when known) added.

NOTES.

Comet Notes.—The following new elements and ephemeris of Comet 1905 *c* (Giacobini) are by A. Wedemeyer from 23 observations extending from Dec. 6 to Jan. 1. They are all well represented, showing that the orbit cannot differ much from a parabola (*Ast. Nach.* 4067):—

T	1906 Jan. 22·4022 Berlin M.T.	
ω	199° 15′ 28″	1906·0.
☊	92 4 31	
i	43 39 16	
log q	9·33334	

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* 'Our Stellar Universe: Stereoscopic Star Charts and Spectroscopic Key Maps.' By Thomas Edward Heath. King, Sell, & Odling, Ltd., London. Price 10*s*. net.

Ephemeris for Greenwich Midnight.

	R.A.	S. Dec.		R.A.	S. Dec.
	h m s	° ′		h m s	° ′
Feb. 3....	22 41 4	24 15	Feb. 19....	0 56 9	12 38
7....	23 22 12	21 48	23....	1 19 13	9 35
11....	23 58 5	18 52	27....	1 39 45	6 55
15....	0 29 13	15 44	Mar. 3....	1 58 5	4 23

The comet sets about 5.30 P.M. on Feb. 3, 8.50 P.M. on Mar. 3, its brightness on these days being 4·61, 2·55 respectively, that at discovery being unity. It may be faintly visible to the naked eye in mid-February. Photographs were obtained at Greenwich early in January, which showed that it was then very bright (probably at least 3rd mag.), with a tail some 2° in length.

Another bright comet (*a* 1906) was discovered by Mr. Brooks at Geneva, U.S.A., its place being R.A. $16^h\ 19^m\ 28^s$, N. Dec. 47° 10′ at G.M.T. Jan. $26^d\ 15\frac{1}{2}^h$. The motion is simply described as North-west. It will be seen that the comet is circumpolar, and therefore observable throughout the night, though it is most favourably situated after midnight.

A. C. D. C.

MINOR PLANET NOTES.—As not infrequently happens, the new year has opened with a rush of discoveries, and at the present rate another couple of years will suffice to exhaust the two-letter designations, after which it will be necessary either to begin afresh or to employ combinations of three letters.

Planet.	Discoverer.	Date.	Place.	Magnitude.
SE ..	Wolf.	1905 Dec. 17	Heidelberg.	12
SF ..	Metcalf.	Dec. 5	Taunton, Mass	12
SG ..	Wolf.	Dec. 27	Heidelberg.	13·5
SH ..	Metcalf.	Dec. 24	Taunton, Mass.	13·5
SJ ..	Kopff.	1906 Jan. 15	Heidelberg.	12·5
SK .	Kopff.	Jan. 15	Heidelberg.	12·0
SL ..	Wolf.	Jan. 22	Heidelberg.	11·8
SM .. SN ..	Details not yet to hand.			
SO ..	Kopff.	Jan. 23	Heidelberg.	11·6

SO was moving 23′ North daily; its orbit is likely to prove interesting.

A. C. D. C.

OBITUARY.—CHARLES JASPER JOLY. It is unfortunately a coincidence that the Chair of Astronomy in the University of Dublin should have become vacant, and again through death, so soon after a similar vacancy in the University of Edinburgh. C. J. Joly was appointed to the position of Andrews Professor and Director of the Dunsink Observatory, which office carries with it the title of Royal Astronomer of Ireland, in 1897, in succession to Dr. Rambaut, and now he is dead at the early age of forty-one. His tenure of office was too short for him to have made a great name in astronomy, especially as he had not previously associated himself with the science, his genius lying more in the domain of pure mathematics. His 'Manual of Quaternions' is well known, and he edited the third edition of Preston's 'Theory of Light.' Prof. Joly was elected a Fellow of the Royal Astronomical Society in 1898, and of the Royal Society in 1904. He died on January 4 after a long illness.

REV. W. R. M. WAUGH. We have also heard lately of the death of this aged astronomer, which happened on November 25 last. William Robert Maurice Waugh was born in London on 1818 July 25; after his early education he joined the Congregational ministry. He read and lectured on astronomy, and for some time was Director of the Coloured-Star Section of the Liverpool Astronomical Society. He was a member of the British Astronomical Association from its foundation, and was Director of the Jupiter Section, himself using a 4½-inch Wray equatorial and a 12¼-inch reflector. His principal astronomical literary work will be found in the publications of the Association, but he also contributed elsewhere.

THE STELLAR MAGNITUDE OF THE SUN.—The following is the result of a comparison of the light of the Sun with that received from the stars named, made by Prof. Ceraski, of Moscow. The details, published in *Ast. Nach.* No. 4065, show that these results were arrived at by comparing reflected sunlight with Venus in the daytime, and then Venus with the stars after sunset:—

Light from Sun = 290550×10^6 light from Polaris;
" " = 77630×10^6 " Procyon;
" " = 17045×10^6 " Sirius.

If the magnitudes of the three stars be taken respectively as 2·15, 0·56, −1·09, they give for the stellar magnitude of the Sun −26·51, −26·66, −26·67.

THE ST. LOUIS EXHIBITION, 1904.—The report of the British Commissioners for this Exhibition, which is just to hand, states that justice was scarcely done to some British exhibits. The Juror for photography has to express his regret that the fine astronomical transparencies from the Royal Observatory and lantern-slides of photo-micrographs (these are probably those exhibited by Dr. Spitta, and for which he received an award) were not displayed so that they could be seen by transmitted light, but were shown in a flat show-case. Other British exhibitors who received awards were the Solar Physics Observatory, Sir William Abney, Sir Howard Grubb, Rev. W. Sidgreaves, Mr. Thos. Thorp, and Mr. A. Hilger.

ERRATUM.—The following mistake in the 'Companion' has been pointed out to us:—Page 10. Occultations: the reappearance of α Leonis on April 5 takes place at 6^h 42^m and not at 6^h 24^m as stated.

THE following is from 'How to Collect Books,' by J. H. Slater:—M. Flammarion has a book bound in human skin. He had admired the skin of a beautiful countess, who bequeathed it to him when she died. This he used for binding a copy of his own work 'Ciel et Terre,' according to the instructions accompanying the gift.

THE next Meeting (Annual General) of the Royal Astronomical Society will take place on Friday, February 9, at 5 o'clock; of the British Astronomical Association, on Wednesday, February 28.

FROM AN OXFORD NOTE-BOOK.

THE award of the Royal Astronomical Society's Gold Medal to Prof. W. W. Campbell, which will receive universal approval, has a specially gratifying feature in that it associates three friends in a delightful way. Hale, Boss, and Campbell, who have been honoured by the Society in three consecutive years, have not only done much for the advancement of astronomy individually, but on more than one occasion have met in conference or in joint enterprise. Moreover, they were, until recently, very symmetrically distributed over the United States, Boss and Campbell on opposite shores (approximately), and Hale in the middle. But, of course, Hale has now spoilt this arrangement by his move westwards, and it is not easy to suggest any method of restoring symmetry with advantage to astronomy. But it is finally worthy of attention, especially of Prof. Boss's attention, that although we put our Annual Meeting at such an inclement season that it is well-nigh impossible to expect our Medallists to be present, yet we have nevertheless had the great pleasure of seeing two members of the trio on this side of the Atlantic recently. *This* state of matters it *is* possible to render symmetrical, and with great advantage to English astronomers at any rate, though we must remember the counter-claims of a *magnum opus*.

The occasion which brought Campbell and Hale to England was, of course, the recent meeting of the Union for Cooperation in Solar Research, the mention of which revives pleasant memories—how quickly they have become memories! Four months have since flown, and yet it seems only yesterday that we were all assembled here in Oxford in the friendliest conference. By the hospitable invitation of the Warden and Fellows of New College the foreign delegates were the guests of the College, and were housed in College rooms, for the most part in those ranged round the old quadrangle within a stone's throw of the houses where Halley and Bradley dwelt. There was an impressive intermixture of physicists with astronomers and meteorologists—a welcome sign that the process of estrangement between different sciences is not necessarily to go on indefinitely, but is to be replaced, in some instances at least, by a process of re-combination. But this variety of representation naturally increased the possibilities for conversation, so that, in spite of all the facilities afforded by close proximity, the days of the Conference came to an end long before the topics for discussion were exhausted.

Besides the regular Sessions of the Conference there were, of course, numerous Committee Meetings, formal and informal, sometimes at breakfast, sometimes after dinner. One important Committee was appointed on a day fully occupied by formal sessions, and was nevertheless instructed to report on the following morning. Accordingly they quietly withdrew after dinner and heroically commenced their deliberations. But the protracted absence of such important people gradually attracted attention, and as the hour grew later the writer was commissioned to make enquiries. He tracked out the committee-room and witnessed an interesting spectacle. The table was littered with abortive attempts at the formulation of resolutions. The Chairman sat with a face of grim determination, his right hand free for the manipulation of a cigar, but his left hand firmly planted on one single slip of manuscript, almost illegible with erasures and corrections, but representing a veritable triumph—the one resolution which had actually been passed. Some of his colleagues were pacing the room in the wrestle with ideas, while others were seated in a closer grapple with actual pen and paper. From time to time one of these latter ventured a request for reference to the resolution already passed, perhaps even timidly pulling at the paper to test the pressure exerted by the Chairman's left hand; but the hand was firm, and the voice was also firm: "C'est fini, çà!" After gazing and listening in admiration for some minutes, I was fain to leave them to continue the struggle, which was ultimately successful. But I can only imperfectly gauge the Chairman's feelings when, after the Committee had actually, and somewhere about midnight,

produced a complete set of resolutions, the suggestion was ma
next morning to "refer them back to the Committee for furth
consideration"!

In other instances, however, results were reached by a l
arduous route. Especially was this so in the case of the "Wa
lengths" Committee, which, if I am not mistaken, had only
meet to find itself practically in unanimous accord—an acco
which was in no way modified by the important and gracef
addition to their report, moved by M. Perot, as follows:—

> The wave-length of this line shall be fixed permanently, and thereby defi
> the unit in which all wave-lengths are to be measured. This unit shall di
> as little as possible from 10^{-10} metre, and shall be called the Ångström.

The permanent association of the name of a great man with the unit, although in one sense a matter of detail, is certainly not the least important of the things effected by the Conference.

But this is, of course, not the place to give a full account of our proceedings. Indeed, such an account would be a lengthy one, for a good many events were crowded into the week. Not only were we invited to dinner by the Royal Astronomical Society Club, but we sat out in the rain to get photographed. It was an obvious portent, but of mysterious significance, that the rain should fall just as those who were devising a new attack on solar problems sat down to be photographed. One possible interpretation is that we had not sufficiently reckoned with the weather. But the weather did not after all beat us in this case, for the photograph was successfully taken; and there are Janssen and Lockyer, the heroes of 1868 (whose joint discovery, in the words of Professor Hale, marks the origin of all work with the spectroheliograph), seated in the place of honour; and there, too, is Hale himself, and Deslandres, and Kayser, and Fabry, and Perot, and Schuster, and Ångström, and all the others—really a picture worth having. We saw it for the first time at the delightful "At Home" given in London by the President of the Royal Astronomical Society. And then we all went to Cambridge and had an entirely new set of experiences there.

Among many hospitable things done for us at Cambridge, there was a very pleasant dinner in the Hall of King's College; and the Provost made a speech of welcome in fluent French, to the obvious gratification of many of our visitors. I wish I could more exactly record here the speech made later by Professor Campbell. He first told us a story of one of the numerous visitors to the Lick Observatory, who was dissatisfied with the apparent size of the planet Saturn, and wanted a higher magnifying-power put on the telescope. It was replied that the power in use was judged most suitable for the great majority of those present. "But," objected this particular visitor, "I have come some thousands of miles to "see your observatory, and I hoped to see it at its best. I think "you ought to do something a little extra for the *better class* of "visitors." Professor Campbell then made skilful use of the incident, which he told much better than my poor memory enables me to tell it, to include all those present in the "better class of visitors," for whom, if they would only present themselves at the Lick Observatory, he was prepared to do something a little extra. And, from personal experience, I know how big that little is.

PROFESSOR HALE gave us a memorable lecture at Cambridge; and we managed to secure him again for an evening meeting of the Royal Astronomical Society. The pictures he gave us on the screen of the Mount Wilson Observatory, and of the marvellous work being initiated there, could scarcely be surpassed, if even equalled. It was brought home to us that though we had had practically a week for the Conference, the time had been far too short to reap the full benefit of such a gathering. Many of these wonderful pictures—of which we only had a momentary glimpse—were clearly worth careful study. And others, too, had collections: certainly M. Deslandres had a number of excellent and important photographs, which some of the delegates had an opportunity of seeing, though others were less fortunate. We must hope to renew and improve acquaintance with them at the next meeting of the Union at Meudon, in September 1907.

ONE of those who by some unfortunate mischance did not get to the Conference, although he was expected, was Mr. Hansky; and he has since given us fresh reason to regret his absence, for the large scale photographs of the solar granules obtained by him at Pulkowa are simply fascinating, and represent a well-deserved success for an enterprise pursued with considerable determination. Finding that the Sun's surface changed so rapidly that no similarity could be found in two pictures taken a few minutes apart, Mr. Hansky arranged an apparatus for taking 8 photographs at the short interval of 15–30 seconds, on the scale of 2 feet to the Sun's diameter. Magnifying these again ten times, the little granules (the "willow leaves" or "rice grains" of the controversy of half a century ago) are about half an inch by a quarter: or in seconds of arc $4'' \times 2''$; say 2000 × 1000 miles. But the important point is that those in one picture can now be recognized in the next. The changes are rapid, for the granules are moving with velocities of the same order as that of the Earth in its orbit (say 20 miles a second, or their own diameter in about a minute), and they coalesce and separate in a bewildering manner. But Mr. Hansky has shown that it is possible to follow their history if only we can find the requisite patience. There seems to be no doubt about the possibility and there is certainly no doubt about the patience. The work necessary for a complete survey of solar phenomena was recently multiplied by a large factor, when Prof. Hale's spectroheliograph showed us that we must regard the region of investigation as having three dimensions instead of two; and now Mr. Hansky has multiplied it again by showing us the great pace at which we must ride to follow any one of these granules across country. On the whole it would seem that there is still something left for astronomers to do.

About a year ago Mr. H. C. Plummer pointed out the need for examining the computations of cometary orbits for traces of the effect of light-pressure (see Mon. Not. R. A. S. lxv. p. 231). Recent work, especially the well-known paper of Dr. Poynting, has drawn attention to the possibility of detecting the pressure which the Sun's radiation undoubtedly exerts, as Maxwell first remarked, and which has the effect of slightly altering the Sun's apparent mass. It now appears that an important step was taken in this direction more than a dozen years ago. We learn from *Astr. Nach.* No. 4062 that the late Dr. Svedstrup, who died in 1893, left behind him some computations of the orbit of Comet 1886 I. which show clearly the necessity of diminishing the apparent mass of the Sun by ·000083 of itself. Dr. Svedstrup found it impossible otherwise to satisfy the observations. This would correspond, Mr. Plummer tells me, to the Sun's light-pressure on particles of about 3 mm. diameter, if their density be 6. It is to be hoped that, in the light of this result, other cases will be examined; and if confirmation is forthcoming we shall all agree with Dr. Thiele and Dr. Winther, who have brought these interesting computations to light, as to the merit of Dr. Svedstrup's work and the honour due to his memory.

In Vol. viii. of the *Astronomische Nachrichten*, dated September, 1830, we read that King Frederick VI. of Denmark presented a gold pocket chronometer to the great traveller von Humboldt, in recognition of his services to geography. The watch was made by Kessel and inscribed

FREDERIK DEN SJETTE TIL ALEXANDER V. HUMBOLDT.

Owing to circumstances which need not concern us, this interesting relic is now for sale. It still goes well as a timekeeper and its rates up to the year 1894 are on record. Possible purchasers should communicate direct with Prof. H. Kreutz, Herausgeber der Astr. Nachrichten, Kiel, Germany.

368.] MARCH, 1906. [*Price* 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

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ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1906 February 9.

Mr. W. H. Maw, *President*, in the Chair.

Secretaries: E. T. Whittaker, M.A., F.R.S., and
T. Lewis.

Mr. Lewis read the Minutes of the last Annual General Meeting, which were confirmed.

Mr. Levander presented the Report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1905, and found and certified the same to be correct. The cash balance in hand on 1905 December 30 was £303 12*s*. 10*d*., as compared with £262 5*s*. 2*d*. on 1905 January 1; but the Auditors called attention to an overdraft from the bankers of £400, due mainly to the cost of re-decoration of the Society's rooms, which appeared on the credit side of the account. Sixteen hundred pounds, which formed part of the McClean bequest of £2000, had been invested in Swansea Corporation Stock. The Auditors had inspected the books, instruments, and other effects in the possession of the Society, which appeared for the most part to be in good condition but they called attention to certain instruments or parts of instruments lying in the cellars which in their opinion might be disposed of with advantage.

The President then announced that Mr. Whitelaw Reid, the American Ambassador, who would receive the Medal on behalf of Prof. Campbell, was already within the building, and suggested that, contrary to usual custom, the awarding of the Medal should take place before the presentation of the Report. This was acceded to, and the American Ambassador, escorted by Sir William Huggins, entered the room.

The President announced that the Gold Medal of the Society had been awarded to Prof. William Wallace Campbell, Director of the Lick Observatory, for his spectroscopic researches, which have greatly increased our knowledge of stellar motions. He then gave a comprehensive account of Prof. Campbell's work, dealing successively with the early history of the spectroscopic measurement of stellar velocities in the line of sight, with Prof. Campbell's design of the Mills spectrograph at the Lick Observatory and the important work done with that instrument, including his discovery of many spectroscopic binaries, and his preliminary determination of the Sun's motion in space from data founded on his spectroscopic observations. Prof. Campbell's researches in other branches of astronomical spectroscopy were also referred to. In conclusion he said:—

"It is a matter of regret to us all that it has not been possible for Prof. Campbell to be with us this afternoon to receive in person the Medal which has been awarded to him. But in view of the fact that it is only a few months since he visited Europe, and of the important work which he has in hand at Mount Hamilton, Prof. Campbell's absence, although, as I have said, a matter for regret, is not one for surprise. Moreover, we are not without a compensation. It has on many occasions been our pleasure to heartily welcome in this room Mr. Choate, for so many years the American Ambassador in this country, and now Prof. Campbell's absence has afforded us the opportunity of extending an equally cordial welcome to Mr. Whitelaw Reid, who has succeeded Mr. Choate in his most important office, and who will, I am sure, also succeed him in the esteem and regard of the British nation.

"There is a special fitness in Mr. Reid's presence here to-day, as I have learned that—by a peculiarly happy coincidence—he is the son-in-law of Mr. D. O. Mills, through whose munificence it was rendered possible not only to construct the Mills Spectrograph but also to organize and carry out those observing expeditions to Chile which have proved such an extremely valuable aid to Prof. Campbell's scheme of research.

"Mr. Reid, in handing you this Medal which you have kindly undertaken to transmit to Prof. Campbell, I will ask you to assure him of the very deep interest which we in England take in the important work which he is so successfully prosecuting, and to express to him our high appreciation of the influence which his researches have exerted—and still are exerting—on the development of astronomical knowledge."

Mr. Whitelaw Reid. It is a great pleasure to me to be the medium of transmission of this very high token of your appreciation of the labours of my distinguished fellow-countryman. It gives me great pleasure to take part in the ceremony to-day, and to hear the learned address of the President expressing such generous appreciation of Prof. Campbell's work. Prof. Campbell will value this decoration as highly as any statesman would value a decoration awarded to him by his government for distinguished political services. On his behalf, and in the name of the United States, I beg to express my profound thanks, and I think I may add the thanks of the staff of the Lick Observatory, for this honour conferred on its Director. My country is always proud of any advance in art or science made by her sons—prouder far of that than of triumphs in commerce or war. The American people, therefore, will be highly gratified at this mark of your Society's approval, especially as it comes from a country with which we are closely related, and to the opinion of which we are wont to attach great importance.

Mr. Whitelaw Reid then left the Meeting.

The Secretaries then presented the Report of the Council to the Society, and in doing so Mr. Whittaker called attention to the fact that the Society had, for the first time, attained a membership of over 700, the actual number of Fellows in the list now being 709; he also mentioned the McClean Bequest, which had been received during the past year.

Mr. Whittaker read extracts from the obituary notices of Dr. Copeland, Prof. Joly, and Lord Grimthorpe. The complete list contained the names of the following Fellows:—T. M. Almond, W. E. Besley, G. C. Bompas, Ralph Copeland, Edward E. Crofton, Edward Crossley, John Dansken, Rev. A. S. Farrar, Lord Grimthorpe, Rev. T. Harley, Charles Horsley, Rev. S. J. Johnson, Lieut.-Col. H. S. G. S. Knight, T. E. Knightley, G. R. Stevens, Rev. W. R. Waugh, and Admiral Sir W. J. L. Wharton, and the three Associates, Paul Henry, Pietro Tacchini, and Otto Struve. Prof. C. J. Joly's name was added although he died in January of this year.

Mr. Lewis read extracts from the Reports of Observatories, which included, besides the Royal and University Observatories, those belonging to the following amateur astronomers:—W. E. Wilson (Daramona Observatory, Westmeath), Mr. Saunder (Crowthorne), Rev. T. E. Espin (Wolsingham Observatory), and the Rousdon Observatory (*Observer*, Mr. Grover).

Mr. Lewis read the list of the subjects reported on in the Progress of Astronomy during 1905;—Variable Stars, The Comets of 1905, Minor Planets, New Satellites, Meteoric Astronomy, A New Reduction of Groombridge's Catalogue, Total Solar Eclipses, The International Union for Co-operation in Solar Research, Double Stars, Solar Activity in 1905, The Astrographic Chart and Catalogue.

On the proposal of *General Waterhouse*, seconded by *Major Kingsley-Foster*, it was resolved that the Report of the Council be received and adopted, and that it be printed and circulated in the usual manner, together with the President's Address and the Report of the Auditors.

The President appointed Mr. Inwards, Capt. B. F. Stevens, and Major Kingsley-Foster Scrutineers of the Ballot for Officers and Council.

The Ballot was then taken and the result announced by Major Kingsley-Foster as follows:—

President: W. H. Maw. *Vice-Presidents*: Prof. F. W. Dyson, J. W. L. Glaisher, H. F. Newall, Prof. H. H. Turner. *Treasurer*: Major E. H. Hills. *Secretaries*: T. Lewis and E. T. Whittaker. *Foreign Secretary*: Sir William Huggins, K.C.B., O.M. *Council*: Sir William Abney, Sir R. S. Ball, Sir W. H. M. Christie, Bryan Cookson, P. H. Cowell, A. C. D. Crommelin, Alfred Fowler, A. R. Hinks, E. B. Knobel, Major MacMahon, S. A. Saunder, E. J. Spitta.

It was proposed by *Mr. Cookson*, and seconded by *Mr. W. W. Bryant*, that the thanks of the Society be given to the Vice-Presidents and Members of Council who now retire, for their services in promoting the objects of the Society.

It was proposed by *Mr. Conrady* and seconded by *Prof. Turner*, that the thanks of the Society be given to the Auditors of the Treasurer's Accounts and to the Scrutineers of the Ballot.

The following gentlemen were elected Fellows of the Society:—

Robert Courtenay, B.A., 34 Wilmount Street, Woolwich.

Lieut. Chetwode G. G. Crawley, R.M.A., B.A., H.M.S. 'Vernon,' Portsmouth.

John Milne Gardiner Shaw, Mem.Inst.N.A., F.R.Met.Soc., c/o John Swire & Sons, 8 Billiter Square, E.C.

The following Candidates were proposed for election as Fellows of the Society:—

Rev. Walter Briscombe, Hillam Royd, Abbey Park Road, Grimsby (proposed by R. Wilding).

Dr. Edalji M. Modi. F.C.S., &c., Sleater Road, Bombay, India (proposed by J. D. Bharda).

A. S. Eddington, B.A., Trinity College, Cambridge (proposed by E. T. Whittaker).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1906 January 31.

A. C. D. CROMMELIN, *President*, in the Chair.

Secretary: J. A. HARDCASTLE.

THE Minutes of the previous Meeting were read and confirmed.

Among the presents announced was a little book, compiled by M. Moye, upon one of the expeditions for viewing the late solar eclipse, which it was announced could be obtained by Members at the price of *1s. 3d.*, and a 2-inch transit-instrument by Troughton & Simms, presented by Mr. W. Heath.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of four new Members was unanimously confirmed.

The President said that, before the Meeting proceeded to consider the papers received, he wished to say a word concerning the death of the Rev. W. R. M. Waugh. They would all be sorry to hear of the death of the veteran astronomer, who had done so much work for the Jupiter Section of the Association, and he was sure they would wish to associate themselves with the Council in a vote of sympathy and condolence with the family of Mr. Waugh. He was sorry also to say that Mr. Thwaites, who was compiling the Eclipse Report, was in such bad health that he had been compelled to hand the work over to some one else. This would mean considerable delay in bringing out the Report. He (the President) much regretted the cause of the delay and the delay itself.

The Rev. T. E. R. Phillips, Director of the Jupiter Section, read a note by *Dr. R. D. Givin* describing an observation of the reappearance of Jupiter's 2nd satellite (1905, November 4). The writer said: "When the satellite had got quite clear from the limb an impression of flattening of the limb was conveyed to the eye." Mr. Phillips commenting on this remarked that both he and Mr. Denning observing together at Bristol had experienced precisely the same illusion on January 3. Bad seeing had nothing to do with it, because definition was absolutely perfect.

A paper by *Mr. A. Stanley Williams*, on "Jupiter's Satellite II. and its Shadow," was next read, in which the writer dealt with a question that had given rise to much discussion lately as to whether or not the shadow of Jupiter's Satellite II. was partly hidden or cut off by the satellite itself on the night of November 23 of last year. A very good view of Jupiter had been obtained by the writer on that night, and among other points noted it was observed that "a considerable part of the shadow was hidden or cut off by the satellite."

Two papers by *Major P. B. Molesworth* were also read, on "The Great S. Tropical Dark Area on Jupiter" and "Minor Variations in Rotation-Periods." These were discussed at some length by Mr. Phillips, who illustrated his points by the aid of the blackboard and diagrams thrown on the screen.

Mr. G. J. Newbegin said that he also had experienced the illusion of a flattening of the limb of Jupiter at the approach of a satellite.

The President said the different notes communicated by Mr. Phillips had raised many interesting points. The apparent flattening could only be an illusion, but it was certainly a rather curious illusion, and it was difficult to see a cause for it.

Mr. E. W. Maunder was asked by *The President* to make a statement regarding Giacobini's Comet.

Mr. E. W. Maunder said they had the good fortune to have two comets under observation at the present time. One was discovered only two or three days ago—on Saturday morning in Greenwich time—by a former Member of the Association, Prof. W. R. Brooks, of Geneva, New York, who found a bright comet near τ Herculis. It was at present going north at a pretty rapid pace. and was about the 8th magnitude. It was well situated for observation, and might be observed at any time during the night. It had been photographed at Greenwich Observatory.

A photograph of Giacobini's Comet, taken at Greenwich on January 8 before it passed perihelion, was thrown on the screen. It was taken in bright dawn, just before sunrise. The longest tail was 1½ degrees in length, and on the original photograph four distinct tails could be made out.

Mr. Maunder then gave an abstract of a paper on "The Solar Origin of Terrestrial Magnetic Disturbances." He referred in the first place to some criticisms of his papers by Mr. G. D. Hirst, the President of the New South Wales Branch of the Association, and pointed out that Mr. Hirst appeared to have entirely misconceived the purport and results of his papers. Until his (Mr. Maunder's) first paper appeared it was generally held that there were only two possible alternatives—either there was no connection between any solar activity and our magnetic disturbances, or, the solar action, giving rise to those disturbances, came from the Sun as a whole and was radiated in all directions through space. Now it had been shown that our magnetic disturbances themselves had characteristics which proved indubitably (1) that they were connected with the Sun; (2) that the Sun's action, of whatever nature, was not from the Sun as a whole, but from restricted areas; (3) that the Sun's action was not radiated, but restricted in direction. That view had been accepted, explicitly by many, implicitly by all.

The President said they had always been very interested in Mr. Maunder's expositions of the theory of magnetic disturbances, and his explanation that night had made the subject still clearer. The quotation he had given from Prof. Dyson had shown how greatly the odds were in favour of Mr. Maunder's main contention. Part of his original theory, of course, dealt with what were more or less speculative subjects, but the main contention about the 27-day period was, he thought, pretty well a certainty. The President added that he hoped some of the Members of the Association would be able to observe one or other of the two comets now on view. As regarded Giacobini's, the ephemeris by Dr. Smart was in the current number of the Journal.

Mr. W. T. Lynn read a paper on "Some Ancient Eclipses of the Sun and their Significance."

The President said Mr. Lynn had put in a very clear manner the use which could be made of the old records. Mr. Cowell had been presenting a series of papers to the Royal Astronomical Society, and there was something of a controversy going on between him and Prof. Newcomb as to the weight to be attached to these old eclipses. Certainly it was very interesting to find how the records that were made by people so very long ago (some 3000 years ago in the case of the first eclipse) could now be used to correct the motion of the Moon.

Mr. E. Holmes read a paper entitled "Notes about the Spectroscope." He also exhibited on the screen a lantern-slide sent by *Mr. T. Heath,* the author of 'Our Stellar Universe.' The slide was a copy of his stereogram No. 3.

The President remarked that Mr. Holmes had done a great service to those Members of the Association whose purses were not too long, by giving a great many hints as to how a simple spectroscope could be constructed with apparatus which was very easily procurable; and certainly now that the spectroscope was adding so much to their knowledge, both of the solar system and the stellar heavens, it was desirable that as many astronomers as possible should make themselves practically acquainted with that instrument. Mr. Holmes had given them a great many of these interesting little papers that were both useful and very practical.

Mr. Hardcastle read a paper on "The Heart and Nautilus of Venus," accompanied by several slides. He also read notes from the *Rev. H. P. Slade* and *Mr. Alex. D. Ross* relative to brilliant meteors seen at Hull and Glasgow respectively.

The Meeting adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

The usual Monthly Meeting was held on Wednesday evening, February 21, at the Society's Rooms, 70 Victoria Street, Westminster, *Mr. Richard Bentley*, President, in the Chair.

Mr. E. Mawley read his "Report on the Meteorological Observations for 1905." He said that, as affecting vegetation, the weather of the phenological year ending November 1905 was chiefly remarkable for the dryness and mildness of the winter months, the drought and frosts in May, the long spell of hot and dry weather in July, and an exceptionally cold period in October. Wild plants came into flower a few days earlier than usual until about the beginning of May, after which time they were, as a rule, to about the same extent late. Most of the early spring-migrants, such as the swallow, nightingale, &c., reached these shores in advance of their average dates. The best farm-crops of the year were those of wheat, beans, and hops, while barley, potatoes, turnips, and mangolds were all more or less over average. On the other hand, the yield of oats, peas, and hay was almost everywhere deficient, the latter being the worst crop of the year. Apples, pears, and plums were in all parts of the British Isles below average, whereas the small fruits as a rule yielded well. Taking the farm-crops alone, the past year must be regarded as having been on the whole a fairly bountiful one.

The other papers read were:—"Brief Discussion of the General Features of the Pressure and Wind-conditions over the Trades-Monsoon Area," by *Mr. W. L. Dallas*, and "The Dispersal or Prevention of Fogs," by *Dr. W. B. Newton.*

---

*Astronomy in the Old Testament**.

The Delegates of the Clarendon Press have conferred a real benefit upon English readers in publishing a translation of Schiaparelli's little manual 'L'Astronomia nell' Antico Testamento.' The original work appeared in 1903, and was at once heartily appreciated not only in Italy, but also on the Continent and in this country. It was translated into German in 1904, and now has appeared in English, with the advantage of the latest conclusions of its gifted and learned author.

The subject, as treated by Prof. Schiaparelli, proves a wider one, and of greater interest, than might have been expected. His opening paragraph refers to the great debt which we owe not only to the intellectual activity of the Greeks and to the administrative skill of the Romans, but also to the peculiar spiritual endowment of the people of Israel. "It did not fall to the lot of the Hebrew people to have the glory of creating the beginnings of the sciences, or even to raise to a high level of perfection the exercise of the fine arts. . . . Their natural gifts, as well as the course of events, carried them to a different mission of no smaller importance—that of purifying the religious sentiment, and of preparing the way for monotheism."

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* By G. Schiaparelli. Authorized English Translation, with many Corrections and Additions by the Author. Oxford: at the Clarendon Press, 1905.

This mission—even if judged from a purely scientific and secular standpoint—was one of supreme significance; for, as Thiele has recently pointed out so clearly in his 'Theory of Observations,' monotheism is the absolutely necessary foundation for all the physical sciences. Whatever the limitations and deficiencies of the ancient Hebrews, yet in two most important characteristics they stand forth as immeasurably the superiors of any and of all the surrounding nations, no matter how civilized or how formidable in military prowess these were; they refused to deify any of the objects or powers of nature, even the most glorious and exalted; and they held themselves rigidly aloof from the superstitions of astrology and magic. This two-fold attitude, as contrasted with the beliefs and practices of every one of the contemporaneous nations, indicates an intellectual superiority, which, considering the remoteness of the time, can only be characterized as most extraordinary.

This attitude gave the Hebrews an intellectual freedom which would have well fitted them to make great advances in physical research. We have no distinct record of any such advances; probably our author is correct in saying that this glory did not fall to them, and in believing that the entire strength of the nation was expended in the preservation of their great charge of monotheism; and, it may be added, in their long-continued life-and-death struggle with the mighty empires which hemmed them in on every side. On the other hand, it must be remembered that it is only the devotional literature of Israel which has been preserved to us, into which purely scientific enquiries would not enter. Yet even as it is, as our author himself notices, there are some significant hints as to no small scientific activity during the brief "golden age" of the Jewish monarchy under Solomon. Of one thing only can we be certain, that whether the ancient Hebrews studied nature from a scientific point of view or not, no nation, ancient or modern, ever made a more magnificent use of natural objects and natural phenomena in poetry. In this department they were the earliest singers; they still remain unsurpassed for sweetness, unequalled for sublimity.

This sublimity of the Hebrew naturalistic poetry is the first point touched upon by our author, and explains his motive for enquiring "what ideas the ancient Jewish sages held regarding the structure of the universe, what observations they made of the stars, and how far they made use of them for the measurement and division of time. It is certainly not in this field that Jewish thought appears in its greatest originality or power. Yet it is also true that nothing can be indifferent in the life of that remarkable people whose historical importance is certainly not less for us than that of the Greeks and Romans."

Prof. Schiaparelli's next step is the question of the epochs represented by the various books of the Old Testament, and he gives a brief but admirably clear *précis* of the leading results reached by Wellhausen and his successors. These results, he considers, are in the main confirmed by his present enquiry so far as it bears upon them; but he finds few traces of Egyptian or Phœnician influence in the Hebrew notions of cosmography, and less than might have been expected of Babylonian. In this connection he points "to the deeply significant fact that of the five or six names of constellations occurring in the Old Testament not one has been hitherto recognized among the numerous names of constellations found in the cuneiform inscriptions."

The Hebrew conception of the world, if we may trust our interpretation of such allusions as remain to us, was that of a vast body suspended without support in empty space. Its upper part was a great dome or vault, which it is possible, but not certain, they believed to be solid. Stretched out under this overarching roof of heaven was the surface of the Earth, conceived of as a great flat plain; whilst deep within the substance of the Earth was a vast cave, the underworld or "covered place," the abode of disembodied spirits.

Passing from these we come to the heavenly bodies. The poetic references to the Sun are numerous, and many are very finely conceived. Total eclipses both of Sun and Moon appear distinctly referred to in the prophetic writings of Joel and Amos, and Prof. Schiaparelli remarks that three total solar eclipses took place in or near Palestine about their times, or a little earlier. The references to the planets are few and uncertain, but our author considers that the planet Venus may well be intended in the expression in Isaiah xiv. 12, "How art thou fallen from heaven, O Lucifer, son of the morning!" and it is generally understood that "Chiun, the star of your god" (Amos v. 26), is none other than Saturn. Schiaparelli also considers that the Jews paid attention to comets, and that Joel is alluding to bodies of this character in his reference to "blood and fire and pillars of smoke." He further makes the hardly warranted statement that "the appearance of a bolide is *undoubtedly* described in vivid colours in Genesis xv. 17"; and he mentions the idea that Joshua's victory at Beth-Horon was due to a fall of meteoric stones. These few hints give us no real light as to the progress of astronomical observation amongst the Hebrews; there is simply no evidence one way or the other as to whether they had mastered the secrets of the planetary motions, or whether they had not even made them a subject of enquiry. One thing only do we know, that they neither worshipped them nor used them for the purpose of astrological prediction. "It is no small honour to this nation to have been wise enough to see the inanity of this and all other forms of divination. . . . Of what other civilized nation could as much be said?"

The most interesting portion of the book is undoubtedly the enquiry into the exact meanings of the Hebrew constellation names that have been preserved to us. Here Prof. Schiaparelli has been very careful to collect every particle of evidence and to present it as impartially as possible. Perhaps almost too impartially, as the reader is left more or less in the dark as to which is the more probable rendering. The chief passage concerned is that in Job xxxviii., translated in the A. V.: "Canst thou bind the sweet influences of Pleiades, or loose the bands of Orion? Canst thou bring forth Mazzaroth in his season? or canst thou guide Arcturus and his sons?"

There is a strong general consent that *Kimah*, the word rendered Pleiades, has indeed been correctly translated. *Kesil* is not quite so generally held to refer to Orion, partly because, in Isaiah xiii. 10, we have "the stars of heaven and the *Kesilim* thereof." But there would be no incongruity in supposing that the most striking and beautiful of all the constellations should be put poetically as an expression for constellations in general. *Ayish* or *Ash* is not now generally believed to represent the star Arcturus, but rather the two northern constellations over which Arcturus was supposed to watch—the two Bears. The Revised Version therefore gives "the Bear" as the rendering for *Ayish*. It should be remarked, in confirmation of this view, that the name *Benetnaish*, the "daughters of Aish," has actually come down to us associated with the star Eta Ursæ Majoris.

When we come to the interpretation of *Mazzaroth*, Prof. Schiaparelli pleads for a view which has not been generally taken. *Mazzaroth* is usually supposed, and I think correctly, to denote the twelve constellations of the zodiac. The parallel passage in Job ix. gives as the equivalent of *Mazzaroth*, "the chambers of the south," an expression quite analogous to the "mansions" or abiding-places of the Moon of the Arabic and Indian lunar zodiacs. The twelve zodiacal constellations, each in turn the resting-chamber of the Sun, and "brought out" in its own peculiar "season," are, on this hypothesis, set in antithesis to the circumpolar stars of the two Bears, "guided" in their unceasing round in the north. Two of the four quarters of the heavens, the north and the south, are thus marked out. *Kimah* and *Kesil* occur in the Book of Amos in a connection which seems to establish a parallel between the former and morning, between the latter and evening. Now the heliacal rising of the Pleiades heralded the opening of spring some four thousand years ago, whilst the heliacal setting of Orion ushered in the winter. On this view "the sweet influences of the Pleiades" refer to the delights of spring time; "the bands of Orion" to the restraint of winter cold; the two constellations would thus stand for the other two quarters of the sky, the east and the west.

The symmetry of this interpretation is entirely destroyed by Prof. Schiaparelli's explanation, which sees in *Mazzaroth* a reference to the planet Venus, seen sometimes as a morning, sometimes as an evening star. This interpretation rests on the occurrence of a similar word—*Mazzaloth*—in 2 Kings xxiii. 5, where mention is made of those who burned incense "to the Sun, to the Moon, *Mazzaloth*, and to all the host of heaven." Here he concludes that *Mazzaloth* must mean the most brilliant star in the sky after the Sun and Moon, and he refers to the triad of stars found everywhere on monuments in Assyria and Babylonia. Prof. Schiaparelli has, however, missed the exact significance of these symbols. Fortunately within recent years Boundary Stones have been found on which these symbols are distinctly referred to the tutelary deities of the first two months of the year: Sin the Moon-god, represented by a crescent Moon on its back—the position of the new Moon at the spring equinox,—presiding over the first month; whilst the twin deities Samas and Istar presided over the second. The two stellar symbols are not differentiated in form on the earliest monuments, and there can be no doubt, as I have shown elsewhere *, that they are simply representations of the Twin stars, Castor and Pollux, with which the new Moon was in conjunction at the opening of the second month. There is no reason therefore for supposing that Istar is in this connection the planet Venus, but rather the reverse, for that planet, unlike the Twin stars, has no necessary connection with any month of the year. Istar is here rather the female complement of Samas. Further, it seems inconsistent to seek an explanation of *Mazzaroth* in Babylonian symbols, inasmuch as Prof. Schiaparelli has himself drawn special attention to the remarkable fact that no one of the Hebrew constellation names has been hitherto recognized on a cuneiform inscription †.

Two further constellation names are referred to—*Mezarim*, translated "the north" in Job xxxvii. 9, and the "fugitive serpent." Of the former the author makes the ingenious suggestion that if we read for *Mozarim*, *Mizrayim*, we should have "the winnowing fans," and he thinks the two Bears might have had this name from the natural arrangement of the seven chief stars in each constellation. The translation given in our Authorized Version would therefore be substantially correct. The "fugitive serpent" of Job xxvi. 13 he declines to recognize as constellational at all, because the stars of the Dragon do not naturally suggest that figure. Here, I think, his argument is at fault, for it is abundantly clear that the majority of the ancient constellation figures were not suggested by the natural grouping of the stars; and, as I have shown elsewhere *, the numerous serpent forms in the sky all hold very definite and precise astronomical positions, to which they were allotted at a date much earlier than the earliest which is usually assigned to the Book of Job.

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* Monthly Notices R.A.S. vol. lxiv. p. 500.

† A cognate word to *Mazzaroth*, however, does occur in the Fifth Tablet of the "Creation" series; and this, it is clear from the context, should be rendered "constellations," or something nearly equivalent. It has been generally taken by Assyriologists to mean the Twelve Signs of the Zodiac.

The remaining chapters of the book, on the divisions of time amongst the Jews, are full of interest, but need not detain us here. The principal points brought out are the very clear explanation of the curious expression "between the two evenings," that is after sunset, but before complete darkness set in; and the significance of the names used for the different months of the year at different periods of history. This has a greater importance than Prof. Schiaparelli appears to attach to it. The Hebrews themselves used in general no names for the months, but only numbers. Three striking exceptions occur to this rule; the first occurs in connection with the Exodus from Egypt and the institution of the Passover, when the first month of the year, as newly appointed, is named Abib, "the month of green ears," the green ears being offered at that feast. The next is in connection with the building of the Temple under Solomon, so largely carried out by Phœnician workmen. At this time, and here only, the Phœnician month names are mentioned with their corresponding Hebrew numerical equivalents. Lastly, during the exile in Babylon and Persia, and immediately after their return from the captivity, the Babylonian names of the months are in occasional use, often with their Hebrew numerical equivalent.

The concluding chapter on the septenary periods also raises several points of great interest. Prof. Schiaparelli points out the week and the Sabbath of the Babylonians differed importantly from the week and Sabbath of the Jews, being attached to the month, whilst the Jewish reckoning was independent of it. He entirely rejects the suggestion that has often been made, by the late R. A. Proctor amongst others, that the week is due to the astrological significance of the seven planets. Our present names of the days of the week depend upon the division of the *nycthemeron* into twenty-four hours and upon the Ptolemaic order of the seven planets; the twenty-four hour day certainly came from Egypt, where the week of ten, not seven, days was in use; the Ptolemaic arrangement of the planets does not probably much antedate the Christian era. The last Appendix gives an examination of the dates of some 2764 Babylonian deeds and contracts as distributed amongst the days of the month. It is clear from these that the Babylonians did not interrupt their business, as did the Hebrews, upon their Sabbaths; but they marked their Sabbath of Sabbaths, the 19th day of the month, the 49th day as reckoned from the first day of the previous month, in a curious manner. Only 12 contracts bear date of the 19th, whilst the average would be 94. There is, however, a little touch which shows that human nature remains the same throughout the ages, and that the good Scotchman who would only do business on the Sunday by "supposing it were Monday," and the French hotel-keeper who has no room No. 13, had their congeners 3000 years ago in Babylon. For 77 contracts bear the date the "20th *minus* 1." E. W. MAUNDER.

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* 'Knowledge and Scientific News,' 1904, October, pp. 227–230.

The Bielid Meteors in 1905.

[Continued from p. 98.]

AMONG the 67 paths mapped in 21 or 22 hours of effectively clear watch from November 15 to this concluding date of November 27, there were found to have been observed, besides the 37 single-pathed shower-foci already mentioned (consisting apparently about as often of alternative and presumably in large part spurious, as of sure and certain origins), also five very well, and six uncertainly attested double-pathed ones, with three well-shown and two less satisfactorily compounded triple-meteored showers, and lastly three radiant sources (ψ, γ, and ε Taurid showers), with 4, 5, and 6 meteor-tracks conformably assigned to them. Some notes of usefully detailed particulars of these last few conspicuous showers are here given briefly in a Table:—

Showers of Leading Strengths in the Bielid Meteor Period, November 16–27, 1905.

Radiant (in Mr. Denning's General Catalogue).	Dates of the noted meteors, 1905 November.	Number of meteors noted.	Observed mean radiant-point.		Width of radiant-area.
			α.	δ.	°
ε Tauri	16 } 23, 24, 27, 27, 27	5 or 6	60°	+21°	2
γ "	16 (?) } 23, 23, 24, 24	4 or 5 (?)	60	+10	5 or 6
ψ "	24, 24, 24, 26	4	61	+30	4 or 5
38 (*p*) Lyncis	20 (?) } 23, 27	2 or 3 (?)	144	+37	3
ι–κ Ursæ Majoris ...	20 } 23, 26	2 or 3	128	+40	3
α Ursæ Minoris	20, 20, 21	3	55	+86	½

These brightest, and some few next strongest centres of the recent Bielid period, present, with one or two exceptions, only a very partial and indistinctly near resemblance to parallel lists of prominent radiant-points of the Bielid period similarly derived from 100 meteor-tracks mapped in Italy on November 20–30, 1897, by Mr. J. A. Hardcastle *, and further by myself from a collection, for the same period, of 120 meteor-tracks from the British Association's Meteor Reports of 1861–80, along with others mapped at Slough in later years to 1897 †, and lastly from

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* *Nature*, vol. lxi. p. 271, January 18, 1900.

† *Ibid.*—In the list of major radiant-points there given in a Table, those designated ζ Persei, *y* Orionis, and *k* Lyncis, should have been named, by their titles in Mr. Denning's 'General Catalogue of Meteor-showers,' ψ Tauri, *o* Orionis, and *o* Ursæ Majoris.—In the list of minor radiant-points given later on the same page, in the text, the position 253° +4°, given as the place of the radiant-point of the shower from near ε Piscium, should have been 353° +4°.

30 meteor-tracks mapped here in November 23–27, 1900, the cases of real parellelism in all of which, with each other and with this last November's path-results, show such great discrepancies from any steady conservation of shower-strength in the main meteor-currents' repetitions, that it looks very manifest and probable that all these weak displays of inconspicuous meteor-currents have maximum and minimum periods of brightness and density from year to year in the same way as the nine or ten major star-showers of the year are found to give us meteor-spectacles of great inconstancy in any considerable series of successive years.

But even in the paucity of meteor-flights here shown (and pointed out at Brighton, U.S.A., by Mr. Sawyer also, in his above-noted interesting account of the nearly total absence, especially, of Biela shooting-stars), which was increased last year by the Moon's considerable dimming of the brightness of both stars and meteors, and which prevents a plentiful collection of meteor-tracks in any one Bielid season from being easily obtained, it is yet of much interest to find the main non-Bielid, ε and ψ Taurid showers, which held the highest places in the above-named pair of lists (combined) of 1897, with percentage proportions 12 and 5, of their paths in the whole meteor-path collection, retaining their pre-eminences in this last year's list with percentages about 8 and 6 of all the collective Bielid and non-Bielid charted tracks. In the list of thirty paths of November, 1900, the same two showers, and that of the γ Taurids, were all three placed first and foremost over other showers, with three meteors each, or with a numerical proportion of 10 per cent. of all the listed paths. The proportion of γ Taurids in last year's list was 7 per cent.; but from the two lists of 100 and 120 paths of 1897, three γ Taurids only, and one ε Taurid, from $57^\circ + 7^\circ$ and $52^\circ + 5^\circ$, could be obtained, with a percentage of less than 1½ for the former shower.

The *p* Lyncids, ι–κ Ursids, and Polarids of last November had no counterparts at all in either of the lists of 1897 and 1900; while of the showers to which the five next prominent positions after ε and ψ Taurids were accorded in the two lists combined together of 1897, those from ι Aurigæ, ο Orionis, and ε ζ Arietis were only marked by one meteor each, and those from δ Geminorum and ο Ursæ Majoris by no meteor-path at all, in the fairly numerous path-list gleaned here in watches for the Biela meteors last November. With the single nearly constant yearly exception of a few chief showers in Taurus, it would thus seem to be a rather depressing, but yet a manifestly true conclusion, that the hopefully projected task of ranging the contemporary general or secondary meteor-showers of the Bielid period in some approximate order of principal strengths or of distinctness of date and situation, is one which must wait for its accomplishment by a much more numerous and long-pursued collection in the Bielid period of new meteor-path projections, until the strongest showers in any year may cease to be, as at present they still intractably oblige us to regard them, in the last degree strange and foreign to any fixed array of preference, or disclosed degrees of pre-eminence thought to have been either traced or pretty certainly discerned among them in previous years!

Results of a watch for the Leonid and Bielid showers in the County of Down, Ireland.—Mr. W. H. Milligan, of Holywood, in that county, has kindly favoured me with the following Table of the watches which he kept for both the Leonid and the Bielid shooting-stars. In reference to the "absence of any result" to enter in the Table, he writes that "the altogether negative character of the record sent, may, nevertheless, perhaps be of use in forming a conclusion on the general question"; and he expresses his own conclusion thus, of the astronomical inference to be gathered from the observations:—"The evidence, I think, strongly tells in favour of theory, and of the computations; and appears to show that these [Leonid and Bielid meteor] swarms no longer cut the Earth's orbit as heretofore."

Watches for the Leonids, 1905, at Holywood, Co. Down, Ireland.

Dates, 1905, November.	Hours of Watch: from, A.M. h m	to, A.M. h m	Remarks.	Results.
14 (morning) ...	3 0	3 30	Sky clear; moonlight; (some scud).	Nil.
15 "	3 0 5 0	4 0 6 30	do. do.	One nebulous meteor from near radiant, at $3^h 30^m$ A.M.
16 "	2 0 5 0	3 0 6 30	do. do.	No meteor.
17 "	2 0	3 30	do. do.	do.

Watches for Andromedids, 1905.

16–18 October.	7 P.M.	10 P.M.	Special short watches.	Nil.
November.				
14–17	......	......	Short watches at intervals.	Nil.
23–24	......	......	Special watches.	Nil.
25–27	......	......	Watches at intervals.	Nil.

The observations show a total absence of radiation from either swarm; the one nebulous meteor observed being no doubt a clino-Leonid pursuing a disturbed course.

It will doubtless be admitted by all who, like Mr. Milligan, had the watchfulness and perseverance to endeavour to convince themselves, by observation, of the entire correctness of an astronomical prediction, that the conclusion to which he was thus led may be regarded now as having been well proved and abundantly established from all sides with vigilant attention by the diligence of good observers. It will also be gratefully acknowledged by them and by all astronomers, that the knowledge we have thus acquired of the real widths of two most typical nascent streams of meteoritic matter in the direction of the Sun's attraction on them, is a most satisfactory and important outcome of the skilful and sedulous labour and devotion with which Dr. Downing has on two occasions of the highest popular and scientific interest carried out to a successful end the long and onerous task of exactly calculating the pretty large transformations in figure and position of those two excentric meteor-currents' orbits, produced by exceptionally strong actions on them, at rather close quarters, of the two nearest to us of the major planets.

Observatory House, Slough, A. S. Herschel.
1906, Jan. 20.

The British Association in South Africa.

The array of Presidential banners never looked so fine as on the beautiful white walls of the City Hall at Cape Town. Orion and the Solar Spectrum shone with unwonted brilliance from the dark ground on which "HUGGINS" is inscribed, and it was a pleasure to be able to tell to several hundred new hearers the old story of how a presidential motto, which runs "Ubi Crux ibi Lux," became translated at a Red Lion dinner to "Ubi Crookes ibi Spooks."

The splendid interest of the Presidential Address itself has already been dealt with in these columns. In Cape Town the new Darwinism caught the popular imagination, and the local poet, with the reservation

"But I am open to confess
I don't entirely understand it,"

hit off the electron theory in some verses which merit preservation:—

"A speck of dust upon my coat
Contains (it is a startling notion)
A thousand little worlds afloat,
In more or less unstable motion.
And when I give my clothes a brush
(I think 'tis a correct deduction)
A million billion planets rush
Like comets headlong to destruction.

"Or take it round the other way
(The truth of our assumptions given),
The concept follows that we may—
We and the Universe we live in—
Be but a speck of dust upon
A coat of some gigantic pattern.
The owner brushes! We are gone!
Good Lord! I hope he is a slattern!"

His Excellency the Governor was delighted with the scientific description of government as a "mode of motion," and promised that we should find some stability in those motions in South

Africa, to which he, on the threshold, gave the Association a hearty welcome in the name of all. His Majesty's Astronomer, in seconding the vote of thanks, referred to "the delight and considerable mystification" with which he had listened to the Presidential Address at Cambridge the year before, and quoted Huxley as saying that the first sign of the mental degradation of a scientific man was that he turned to philosophy; to which the President retorted that a scientific man turned to philosophy was not the same as a philosopher with a wide knowledge and keen appreciation of scientific research.

A feature of the Sectional meetings which filled the next three days was the strength of Section A; and the strength of that Section lay much on the astronomical side. The presidential address celebrated the bicentenary of Edmund Halley's most striking achievement; the papers that followed included many of great astronomical interest. First came the paper by Dr. A. W. Roberts, of which the printed abstract has already appeared in these pages. But the best point is not in the abstract. Dr. Roberts has had the happy idea of determining the size of the orbit of an Algol variable, and hence its mass, from the fact that the times of minima are altered by "light equation." In the case of RR Centauri he finds that the minima come 1½ minutes sooner than if light had infinite velocity—the effects of light equation are separated from those of eccentricity because the former displaces both minima in the same sense,—and he deduces for the mass of this star something between 50 and 300 times that of the Sun. Should this result be confirmed, it will be of great interest, for other indications have pointed rather to the conclusion that the stars are more nearly alike in mass than in any other characteristic *.

Other results, giving densities for Algol variables of the apioidal form (stars in contact), were criticized by Mr. Jeans, who pointed out that it was useless to apply Kepler's laws to such cases; the principal agency in preserving the partial separation of the bodies is not rotation but the fluid pressure across the neck; the very low densities found for these variables may therefore be altogether wrong.

The discussion of Dr. Roberts's paper paved the way for a paper on the theory of star-formation by Mr. Jeans two days later, which produced quite a sensation in the Section, and provided one of the really great occasions of the meeting. He began by remarking that Laplace relied almost entirely on rotation to account for the development of systems, and that subsequent investigations had dealt with incompressible fluids. But he found that as soon as compressibility was taken into account, everything

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* Result in a paper by H. N. Russell, read at the Cambridge Meeting of the B. A. 1904, but published in the Report only in title.

was completely altered, and that what may be called gravitational instability is generally much more effective than rotational. A uniform distribution of gas is unstable, and tends to concentrate about certain points. The subject cannot be treated at present with numerical precision; but he has been able to show by general considerations—

(1) That we can find an expression for the average ratio of the distance apart of these centres to the mass condensed round them.

(2) That if we substitute the observed average distance we obtain a mass comparable with that of the known masses of stellar systems.

(3) That we can predict a relation between the masses and the density of a primary planet which has thrown off satellites *while liquid* which is in fair agreement with the actual ratios in the solar system.

(4) That if rotation alone were operative in producing satellites, it would be necessary for the planet to contract to inconceivable density before the event*.

There was something very impressive about these results, expounded with a masterly air of simplicity, as if there were no particular difficulty in the subject. The President of the Association expressed the profound interest which attached to this new treatment of the subject, admitted the importance of taking compressibility into account, considered that nevertheless research on the homogeneous fluid bodies was still desirable, and proceeded to describe an investigation of Roche's work which he had recently finished. When Roche discovered his celebrated limit, within which satellites could not exist, he did not seem to realize the full importance of what he had done, and he published no details. All that we know is that the limit 2·44 was derived by some graphical process. Prof. Darwin has recently applied ellipsoidal harmonic analysis to the problem, and found two ways of calculating Roche's limit rigorously. The methods give respectively 2·4554 and 2·4552, which are in striking confirmation of Roche's result. Roche had treated an infinitely small satellite. Darwin extended the work to large satellites, and incidentally proved that a pear-shaped figure with a narrow neck is not a possible case.

Those who were in Section A on the morning of this discussion will be apt to look back upon it with peculiar pleasure. It was not only that a theory had been put forward which seemed to bear on its face the stamp of greater truth than did the artificial-looking unconvincing ring-shedding hypothesis of Laplace. The thing could be put to a rough numerical test, and the actual dimensions of systems proved to be very much what might have been expected. One may hope that the discussion will call general attention to the series of memoirs in the *Phil. Trans.*,* where the results are developed in detail, and especially to the last, where "those readers whose interest lies in the application of the results rather than in the processes by which they are obtained may be recommended to turn at once to § 22."

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* See 'Astrophysical Journal,' 1905, September.

Celestial photographers (except those of the western hemisphere) have long regarded the *réseau* as one of the most beautiful and valuable accessories in their equipment. More recently they have been introduced to the idea of using the stereoscopic principle in the measurement of their plates, but have heard that a *réseau* upon those plates may in that case be a cause of embarrassment. They ought, therefore, to take a peculiar interest in Mr. Fourcade's method of photographic surveying, of which the essential features are the use of the *réseau* and stereoscopic measurement. Over the ends of a base 300 metres in length, Mr. Fourcade erects two stands, on which camera, theodolite, and sight-vane are interchangeable. The only adjustment to be made in the field is to set up the camera successively at each end, pointing rigidly perpendicular to the base. The *réseau* is mounted inside the camera so that it can be brought down on a flap over the plate. The measuring-machine, constructed by Messrs. Troughton & Simms to the designs of Mr. Fourcade, has the great advantage over the stereo-comparator that it is entirely differential. The *réseaux* on the two plates are combined to form a background at infinite distance, and the whole picture of the country stands out in relief. The scheme is singularly elegant, and it has been shown to work well in suitable country, of which there is much in South Africa. The greater is the pity that there seems to be little chance at present of its being set to work on an extended topographical survey.

Most astronomers have heard somewhat vague reports of a great address delivered by Prof. Kapteyn at the St. Louis Exposition Congress in 1904, which has not yet been published. One or two have had the privilege of reading the manuscript; but it is curious—and perhaps a warning to others—that to announce a great discovery to a scientific congress may render it inaccessible for a long time afterwards. Even to repeat the substance of it twelve months later, as Prof. Kapteyn did at Cape Town, does not bring it very much nearer publication; for although the Sectional Committee resolved to ask the Council to print the address in full in the Association Report, and the Council agreed, that Report will probably not appear for some months. The consequence of this delay is that, eighteen months after it was first announced, comparatively few people know what "star-streaming" means. The members of Section A at Cape Town are a happy minority, and the writer will be so bold as to set down his recollections of an intensely interesting but rather complicated subject.

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* Vol. 199, pp. 1–53; vol. 200, pp. 67–104; vol. 201, pp. 157–184.

If the Sun were at rest in space, and the real motions of the stars were distributed at random, the proper motions of the stars in a small region of the sky might be represented by lines radiating from a point, and the mean length of the lines in any sector would be independent of the position-angle of the sector. Substitute a mean radius for the bundle of radii in each sector, and the extremities of these radii lie on a circle. Now introduce the effect of the solar motion. Each radius is bent back more or less away from the apex and its length is altered; those in front are shortened, those behind lengthened, and the envelope of the mean radii becomes a kind of oval, whose axis of symmetry is directed to the apex. In part 5 of the publications of the Astronomical Laboratory of Groningen, Prof. Kapteyn has shown how from the shape of this oval it is possible to deduce "the distribution of cosmical velocities," always supposing that their directions are actually distributed at random, and as the result of many pages of disagreeable-looking integration he produced a set of tables all ready to interpret the meaning of the shapes that the ovals would be found to assume when the observed proper motions of the stars were tabulated and the figures constructed. But a great surprise was in store for him when he proceeded to construct the ovals. They did not come out ovals at all, but curious kite-shaped figures, whose principal characteristic was an absurd absence of symmetry about that axis with respect to which they should have been symmetrical. No possible change in the adopted apex of the solar motion gets rid of this dissymmetry, which is most marked about the poles of the Milky Way.

And now for the interpretation. Practically the whole of the stars, according to Prof. Kapteyn, belong to one or other of two great streams, and the *apparent* vertices of their motions lie 140° apart, at points about 7° south of α Orionis and 2° south of η Sagittarii. The true vertices of their motion are at ξ Orionis and the point diametrically opposite it—that is to say, the streams are moving past one another along a diameter of the Milky Way.

In the course of the discussion which followed, Prof. Darwin expressed his admiration of the development which the science of stellar statistics has received at the hands of Prof. Kapteyn, but admitted a grudge against these results, which furnish no confirmation of the theory put forward in his address! Sir David Gill expressed the amazement of the practical astronomer that the systematic character of proper motions should have escaped notice for so long, and was sure that the steps which Prof. Kapteyn advised for the elucidation of this matter would command the best efforts of the best observatories of the world.

There can be no question of the revolutionary character of this discovery, and it will take some reflection to make sure of what is left of the supposed established facts of stellar astronomy. What will happen to the precession constant may easily be guessed. An apex of the Sun's way becomes indefinable when two systems are involved. But perhaps the most interesting point is this: What relation do the stars which have up to the present shown sensible proper motion bear to the stars in general and to the star-clouds of the Milky Way? Kapteyn says that the brighter Bradley stars with proper motion are nearly equally distributed, but that the fainter are condensed on the plane of the Milky Way. Newcomb considers the latter part of this result unreliable, and prefers to say that the distribution of stars whose proper motion is certainly determined bears no relation to that plane. The difference is clearly of extreme importance—just the difference between having found out something of the structure of the whole visible universe and having analyzed into two separate parts the particular star-cloud in which our Sun is involved. For the solution of this and many other points in the theory of star-streaming which are at present obscure, we await with lively impatience the appearance of Prof. Kapteyn's detailed discussion.

A. R. H.

Fireball of 1905 *December* 30.

A MAGNIFICENT fireball, creating a very brilliant effect. though appearing in the strong twilight of early evening, was seen by many observers in Scotland and the northern counties of England on Saturday, Dec. 30 last. The real position of the object was over Scotland in the region of Dumfries and Ayr, and its flight seems to have been directed from E. by S. to W. by N. over a moderate range of the country at a slow rate of motion. But, though not lacking in number of observers, the apparent path of the meteor in the sky could not be very accurately marked, there being few stars visible at the time. The following are some extracts from the observations:—

N. Maclachlan, Cullen, Banffshire.—4^h 25^m. Large meteor three times as brilliant as Jupiter. Came into view about 5° W. of Saturn and Mars, and its course was westwards in a line parallel to the planets referred to. Meteor burst 10° above horizon. Trail 12° or 15° in length, remained straight for 2 or 3 min., then became wavy and assumed a form similar to the usual pictorial representation of lightning. Trail visible 13½ min. in the strong twilight.

E. W., Edinburgh.—4^h 25^m. Meteor of great brilliance. Travelled from the S. in a N.W. direction, and, as seen from the Waverley Bridge, appeared to burst over Princes St. Gardens, a little N. of the Castle Rock. It left a long streak of bright light which the wind blew into various fantastic forms during the 12 min. it remained visible.

F. B., Portlethen.—4^h 27^m. Bright meteor passed down in oblique direction towards S.W. horizon from a point 15° N.W. of the Moon and planets Mars and Saturn. It left a long luminous trail which curled up into an irregular circle and finally faded away 10 min. afterwards. The position was about 25° above the S.W. horizon, so it is probable the Sun was shining on the trail and enabled it to remain so long visible.

W. Blair, Calthwaite Station, near Carlisle.—4^h 26^m. Bright meteor passed over at right angles with Calthwaite Station (10 miles S.S.E. from Carlisle).

W. B. Morrison, Nether Edge, Sheffield.—4^h 35^m. Observer was on the Derbyshire side of Whirlow Bridge, and saw a streaming ball of fire descend diagonally to the Earth. It appeared in the N.W. sky and was inclined at an angle of 50°, thus ↙, disappearing towards W.

T. Buckle, Harrogate.—4^h 26^m. Large meteor, about 1-3rd the apparent diameter of the Moon, disappeared in the N.W. when about 16° to 18° above the horizon.

E. B. Mackinnon, Aberdeen.—Meteor descended at an angle of about 60°, so ↘. It disappeared in the S.W. at an altitude of 15° to 17° above the horizon. Left a streak visible in broken patches for 15 min.

J. B. Butterfield, Radcliffe, Lancashire.—4^h 26^m. Large meteor seen apparently quite near here, and seemed to alight in a field not more than 300 or 400 yards distant.

Gordon Weir, Anniesland, Glasgow.—4^h 25^m. Observer walking from Bearsden to Jordanhill saw large meteor in the western sky falling so ↘ at an angle of 30°, its direction being towards N.W. or possibly N.N.W.

C. A. W. H., Scalby, Scarborough.—4^h 30^m. Riding through Hackness saw a large yellow meteor which fell straight to the Earth in a northerly direction. Duration 3 seconds. Passed behind a hill. Left a trail of light.

Donald R. Curwen, Heversham, Westmoreland.—4^h 25^m. Observer saw a fiery ball heading northward. It broke into a long trail and the head disappeared as a rocket.

J. B. W., Wirksworth, Yorks.—4^h 35^m. Large meteor burst into view in vicinity of the Little Bear's head, and passed towards the N.W. horizon. Colour yellowish. Duration less than half a minute.

At *Cheadle, Staff.*, the meteor was observed near the horizon moving from about N. 33° W. alt. 8° to N. 40° W. alt. 5°.

At *Helensburgh, Dumbarton*, the position was estimated as S. 27° E. alt. 50° to S. 27° W. alt. 30°.

Mr. Alexander D. Ross, at Glasgow, noted the time as 4^h 26^m, and says the path was practically straight, and traversed in a little over 2 seconds. Position from about S. 15° W. alt. 55° to S. 50° W.

alt. 30°. The trail remained visible for more than 12 minutes. The meteor itself was of a bright golden-yellow colour. The trail at first was narrow, yellowish and straight, but it immediately and rapidly developed irregular curves. In a 3-in. telescope it showed a knotted structure. At $4^h\ 35^m$ a large portion of the trail had assumed an elliptical shape.

Observed also at Dysart, Fife, Ilkley, Galashields, Ancrum, Douglas and Elvanfoot, Lanarkshire, and several other places, but no useful details given as to the meteor's course.

It is difficult from the various descriptions quoted to ascertain the real path of the object in a perfectly reliable manner. But there seems strong evidence that the radiant-point was in the N.E. region of Aries, at about 40° +23°. Adopting this place, the height of the meteor comes out about 67 to 27 miles from over Thornhill, Dumfries, to 6 miles south of Arran. The earth-point is indicated some 10 miles N.E. of Rathlin.

The meteor appears therefore to have been a very late ε Arietid, and was possibly a member of the same stream as that which furnished the fireball seen on 1904 Jan. 10 with radiant 43° +22° and height 67 to 31 miles. Many bright meteors have been recorded in various years from the same apparent place in the firmament, and usually in the autumn months; thus the fireballs of 1903 Nov. 18 and 1902 Dec. 2 had radiants at 50° +22° and 43° +22° respectively. A large number of ordinary shooting-stars have also been traced diverging from a similar position during the months from July to December. The recent fireball seems to have been situated at a normal height in the atmosphere, and to have pursued its visible path of 72 miles at a velocity of about 15 miles per second. But the character of the observations, in the particular case we have been alluding to, is not such as to lead up to any very exact deductions, and further data are required to eliminate outstanding doubts. It is to be hoped that these will be forthcoming, for the fireball was one of the most brilliant that appeared during the year, and the contortions of its long-enduring, sunlit trail offered a very curious and striking spectacle.

Bishopston, Bristol, W. F. DENNING.
1906, Jan. 30.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Latin Inscription on the Herschel Obelisk at the Cape.

GENTLEMEN,—

Mr. Hinks takes my correction so genially that I must let him have the fun of "drawing" me about the Latin inscription on the Herschel obelisk.

It really truly was composed A.D. 1905, and afforded much interest and pleasure to those who acted in the collaboration. They were: two of Sir John's sons; two of his grandsons (one still at Westminster), who both have their place in the text; two of his sons-in-law, one being a genuine scholar in languages; and two friends, one fresh from honours in Oxford, and both consulted for their learning. It is gratifying to hear that the result is deemed creditable to the powers of the Latin language.

Six of these have left their impress on the document; and no doubt the critics of a thousand years hence, with their improved methods, will be able to assign the date of composition, and the work done by each "redactor" or "editor," with unerring precision. *At nos audientes*! Yours faithfully,

92 Woodstock Road, Oxford. W. J. HERSCHEL.
1906, Feb. 7.

Halleiana Inedita.

GENTLEMEN,—

As there is a rumour that an English astronomer contemplates writing a biography of Dr. Edmond Halley, I ventured to insert a brief note in *Popular Astronomy* for December 1905 (vol. xiii. pp. 579–580), consisting of a key to printed sources of material. It is well-known, however, that there are many unpublished manuscripts touching the same subject, some of which are mentioned by Miss A. M. Clerke, in her sketch of Halley, in the 'Dictionary of National Biography,' vol. xxiv. pp. 104–109. Among these are the S. P. Rigaud collections in the Bodleian Library; numerous communications by Halley in the letter-books of the Royal Society; and his letters written during his two voyages made in the years 1698–1700. The latter were also cited in the *Observatory*, vol. xxviii. pp. 321–322.

Several unsuccessful attempts have been made by me to establish the existence now of the first posthumous memoirs of Halley, written by Folkes. This valuable manuscript, if it has survived political upheaval, ought to be found in the possession of the Académie des Sciences at Paris. May we hope that it will be recovered in ample time to serve Halley's biographer?

The unpublished portion of Hearne's "Remarks" remain to be examined, as is true also of Halley's commonplace book and some of his original manuscripts said to be in the library of the Earl of Macclesfield at Shirburne Castle, Oxford. There are, too:—(1) some letters by Halley in the Sloane MSS. in the British Museum; (2) MS. life of Halley, ascribed to the Rev. Israel Lyons, in the Observatory library, Oxford; (3) preliminary collections by the author last named, in the library of the University of Cambridge, made with a view to the publication of an edition of Halley's works.

Concerning his ancestry, it is not impossible that Halley, like Newton, may have filed a pedigree in the College of Arms, for which (supposed) document probably no search has ever been made (cf. *Notes and Queries*, London, 9th series, vol. xii. p. 266, Oct. 3, 1903). This memorandum, if it exists, might reveal the origin of his family, and, incidentally, explain why he invariably spelt his given name "Edmond," instead of using the commoner English form of "Edmund."

The foregoing list, which can, perhaps, be augmented, will show how great is the opportunity awaiting an English writer imbued with Halley's achievements. That this opportunity may be seized is a wish that appears, at last, to be within the range of fulfilment. It is a quarter of a century since Captain S. P. Oliver pointed out in the *Journal of Science* (vol. xvii. pp. 91–100) the desirability of a comprehensive review of Halley's life and work. Another writer, at the same time, summed up in these words:—

> It is a fact hardly yet appreciated, either in England or America, that Dr. Edmond Halley is second only to Isaac Newton, whose friend and contemporary he was . . . . and that it is to this close contemporaneity alone that the bright light of Halley's star has suffered diminution of lustre from the brilliant rays of his world-renowned neighbouring luminary. (Cf. *Nature*, vol. xxi. p. 303; London, 1880.)

Later we find this high tribute endorsed by Sir Robert S. Ball, who remarks that:—

> There can be little doubt that the fame as an astronomer which Halley ultimately acquired, great as it certainly was, would have been even greater still had it not been somewhat impaired by the misfortune that he had to shine in the same sky as that which was illumined by the unparallelled genius of Newton. (See *Great Astronomers*, p. 162: London, 1895.)

Very truly yours,

Chicago, U.S., 1905, Dec. 2. EUGENE FAIRFIELD MCPIKE.

The Eclipse of B.C. 431.

GENTLEMEN,—

In the supplementary number of the 'Monthly Notices of the Royal Astronomical Society' (p. 867) Mr. Cowell refers to Prof. Newcomb's article on Ancient Eclipses in No. 3682 of the *Astronomische Nachrichten*, in which he calls in question the reliance that can be placed upon any of the ancient accounts. Would you allow me to point out that in one of these Prof. Newcomb does not quote the account given by the historian (Thucydides) accurately? He speaks of "an eclipse about the year −431, which Thucydides describes as total." The date −431 is not correct; he means that of B.C. 431 or A.D. −430. But that is not the point to which I especially allude. So far from describing the eclipse as total (the central line did not pass over Greece), the historian expressly says that the Sun became *μηνοειδὴς*, *i. e.* crescent-shaped, or, as Ginzel translates it (in the less ambiguous German expression) "sichelförmig." Mr. Cowell gives the original Greek in his paper on the "Secular Acceleration of the Moon's Longitude and Node," p. 861 of the Supplementary number, and I had done so long before in the seventh volume of the *Observatory*, p. 198, under the heading "The Eclipse of Pericles," because Plutarch mentions that that general noticed it and comforted his pilot on the occurrence whilst embarking on his voyage to the Peloponnesus. The historian then deserves credit for the accuracy of his statement; and, as Mr. Cowell observes, his record stands on a different level to that of the eclipse stated by Herodotus to have taken place at a date (B.C. 480) when he could only have been about four years old.

Yours faithfully,

Blackheath, 1906, Jan. 15. W. T. Lynn.

P.S.—The above method of speaking of B.C. dates is indeed generally followed by Prof. Newcomb; but surely it is better, to avoid confusion, to express such dates in the way uniformly done in history, which, as it recognizes no year 0, places B.C. 1 immediately before A.D. 1, so that B.C. 2 becomes mathematically A.D. −1, &c.

Bradley and Pond.

Gentlemen,—

The sixth Astronomer Royal was born five years after the death of the third; and the only reason for coupling them here is that I am about to offer a few discursive remarks about the work of each at the Royal Observatory.

It is well known that Bradley's observations were not published until long after his death: the two volumes in which they are contained appeared in 1790 and 1805 respectively, the first edited by T. Hornsby, and the second by A. Robertson. It was reserved for Airy to reduce the lunar and planetary work from the date (1750) when Bradley obtained and brought into use the greatly improved instruments constructed by Bird. But every astronomer is acquainted with the great debt due to Bessel for the reduction of Bradley's stellar observations, and the formation of the results into a catalogue, which appeared in 1818 in the work bearing the appropriate title 'Fundamenta Astronomiæ.' It is probably not so well known that Maskelyne published, in 1771, as an appendix to the *Nautical Almanac* for 1773, a catalogue of the places of 387 stars, reduced from some of Bradley's observations by Charles Mason, who had been his assistant from 1756 to 1760, afterwards going to America, where he took part in the measurement of the famous Mason and Dixon line between Maryland and Pennsylvania, returning to England in 1768. The epoch of this catalogue is 1760, and it is reprinted by Hornsby in the Introduction to the first volume of Bradley's observations. Maskelyne tells us that the right ascensions of 15 of these stars (13 of the first, and two of the second magnitude) were settled by comparison with the Sun about the equinoxes by the mean of 1175 observations*, and these were the racial points by which the right ascensions of all the other stars were reduced. Of the rest of the 372 stars, he only tells us that they include all stars down to the fifth magnitude that can ever be eclipsed (*i. e.* occulted) by the Moon in any part of the globe; and these stars are marked by an asterisk.

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* Aldebaran 21, Capella 56, Rigel 88, α Orionis 129, Sirius 136, Castor 19, Procyon 119, Pollux 34, Regulus 63, Spica 74, Arcturus 70, Antares 36, α Lyræ 129, α Aquilæ 154, and α Cygni 47.

I have now something to say about Pond's observations, some of which led to a good deal of controversy. Bessel, in the 'Fundamenta,' speaks of him as "*Pondius*, dignus *Bradleii* successor." Olbers, however, writing to Bessel under date 1822, August 4, remarks:—"Auf Pond's Genauigkeit baue ich, unter uns gesagt, nicht sehr, und halte Greenwich durch ihn nur schlecht besetzt." An attack was made upon the accuracy of part of them in the *Philosophical Magazine*, to which Bessel replied in *Ast. Nach.*, No. 84. But it seems to me that he only points out that the author of the attack was not aware of the number of casual errors and errata that are inevitable in reducing and printing so large a mass of observations, adding his own belief that they were on the whole as accurate as could well be made, regretting only that sufficient attention had not been paid to the meteorological instruments and their position.

May we reconcile the different views about Pond's observations by remembering (what is dwelt upon in Mr. Maunder's interesting book on the Royal Observatory) that, after his early years, his absences from the Observatory were long and frequent, the consequences of which may well have been serious.

Yours faithfully,

Blackheath, 1906, Feb. 6. W. T. Lynn.

P.S.—Some of the copies of the early volumes of the *Nautical Almanac* do not contain the Appendixes, of which one is referred to above. That in the British Museum for 1773 has not the table of some of Bradley's stars reduced by Mason. Neither has the volume for 1774 the table of Moon's places reduced from Bradley's observations after 1760, of which, as well as the other, Maskelyne speaks in the Preface to the *N. A.* for 1773. The copy of those Almanacs in the R. A. S. Library contains the latter (the Moon's places), but not the former (the star-places). That in the library of the Royal Observatory contains both. Hornsby gives the star-places as Table IX. in his Introduction to the first volume of Bradley's observations, without any explanation or reference to Mason.

OBSERVATORIES.

Harvard.—Prof. Pickering's annual reports bear a great likeness to one another. They all record a great deal of work, mainly on Variable Stars and kindred research; and because there is so much it seems inexpedient to attempt a summary in limited space of his Report for the year ending 1905 September, but some points of interest may be noted.

Eighteen eclipses of Jupiter's satellites and eight occultations of stars by the Moon were successfully photographed. Three of these occultations were photographed on a plate revolving in its own plane—the apparatus, in fact, forming a chronograph of which the stellar image is the pen.

The minor planet Ocllo discovered at Arequipa in 1901, which is an object of especial interest, since, as the eccentricity of its orbit is the greatest known, it forms a connecting-link between the asteroids and periodic comets, was thought to be in danger of being lost, because attempts to observe it in 1903 and 1904 proved futile. Several photographs of this planet were obtained in 1905 which will determine its future path with certainty.

Publication of Variable Star observations is proceeding. A provisional catalogue of 1227 variables was published by Miss Cannon in 1903 and two others have appeared since then, with the intention of publishing a complete catalogue early in 1906. It appears that about 225 variables were known in 1886 when the photographic work at Harvard began. Since then about 555 variables have been discovered elsewhere and 2197 at this Observatory.

Reference is made to the work by means of kites carried on at the Blue Hill meteorological observatory. Of 17 kite flights the average of the highest altitudes attained was 6940 feet above sea-level, and the actual maximum 11,180. Observations with *ballons-sondes* were made in continuation of a former series, and in one of the 35 ascensions the extremely low temperature of −111° Fahrenheit was recorded at a height of 48,700 feet. In another series of experiments the existence of the return trade-wind, blowing from a general southerly direction at a considerable height, was demonstrated by the trajectory of the balloons.

Another item of the report which is also familiar is Prof. Pickering's request for a little more money. He points out that the anonymous gift of $20,000 in 1902 was advantageously expended in building and in buying the Common Reflector and shows how the sum of $2500 from the Carnegie Institution in 1903 was used, but now these are all gone and he wants more. The observatory, he says, is just paying its way and, as is the case with a company or business in similar conditions, a comparatively small sum would enable its usefulness to be much extended. But is it quite dignified for the science to have to go begging?

NOTES.

Comet Notes. — The following elements of Brooks' Comet (*a* 1906) are by M. Ebell from observations on Jan. 27, 29, 31

He states that the comet cannot be identical with either *d* 1905 or *e* 1905 announced by Prof. Lowell:—

T	1905 Dec. 20·02 Berlin M.T.	
ω	87° 1′	1906·0.
☊	285 39	
i	126 45	
log *q*	0·0 5	

Ephemeris for Berlin Midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Mar. 2....	5 52 47	66 1	Mar. 10....	5 43 6	56 15
4....	5 48 50	63 19	12....	5 42 22	54 13
6....	5 46 7	60 47	14....	5 42 2	52 20
8....	5 44 18	58 26	16....	5 41 58	50 35

The brightness on March 16 is $\frac{3}{10}$ of that at discovery, when Prof. Barnard estimated that it was of 9th magnitude. He described it as large, round, and very diffused, gradually brighter in the middle, where there was an ill-defined nucleus of 12½ magnitude.

It is curious that this comet, like *b* 1905, passed very close to the North Pole, its declination on Feb. 17 being 84° 50′.

The R.A. of the ephemeris should be diminished by $1^m 40^s$, the declination by 50′.

The following is a continuation of the ephemeris by A. Wedemeyer of Giacobini's Comet (*a* 1905), for Berlin midnight:—

	R.A. h m s	Dec. ° ′		R.A. h m s	N. Dec. ° ′
Mar. 4 ..	2 2 13	4 0 S.	Mar. 20....	2 56 52	3 22
8 ..	2 17 55	1 51 S.	24....	3 7 51	4 46
12 ..	2 32 6	0 4 N.	28....	3 18 6	6 1
16 ..	2 45 1	1 49 N.	Apr. 1....	3 27 42	7 10

The brightness on March 4 is 1·05, on April 1, 0·23.

No observations of this comet since its conjunction with the Sun are yet to hand. The weather in England has not been very favourable for its observation.

Ast. Nach. 4072–3 contains definitive elements of Comet 1742 I. by Dr. B. Cohn:

T	1742 Feb. 8·2032 Paris M.T.	
ω	328° 3′ 5″	1742·0.
☊	185 36 10	
i	112 58 50	
log *q*......	9·88409	

The residuals of the normal places are rather large, about 1′, but

Dr. Cohn thinks that the observations are too rough to justify the assumption that the orbit was not parabolic. In any case it cannot be identical with the comet of 1702, as was at one time thought probable.

Dr. Cohn discusses the question how many comets were visible in 1742. He thinks there were probably three, of which the first and second were discovered by Grant in Paris, the third by Dutch navigators in the Indian Ocean. A. C. D. C.

MINOR PLANET NOTES.—The following planets have been discovered, the place of discovery being Heidelberg, except for SP, which was discovered at Vienna:—

Letter.	Date.	Discoverer.	Magnitude.
SM	1906 Jan. 22	Wolf.	12·3
SN	22	,,	14·0
SP	1905 Dec. 31	Palisa.	13·0
SQ	1906 Jan. 24	Wolf.	12·8
SR	24	,,	11·8
SS	24	,,	12·9
ST	24	,,	13·0
SU	24	,,	13·2
SV	24	,,	12·9
SW	24	,,	13·0
SY	15	Kopff.	12·0

SP, SY were at first mistaken for RZ and 352 Gisela respectively, but were afterwards found to be distinct from these.

The identity of RK with 509 Jolanda has been established. RT, SX are identical with 477 Italia, 88 Thisbe.

Ast. Nach. 4073 contains a fine series of observations of 433 Eros made at Florence by Prof. Abetti last July and August. The results are compared with Dr. Witt's ephemeris in the 'Berliner Jahrbuch,' the average values of "Observed—Calculated" being $-0^{\mathrm{s}}\cdot14$ in R.A., $-1''\cdot0$ in Dec. A. C. D. C.

MR. NEWALL ON "ECLIPSE PROBLEMS AND OBSERVATIONS" AT THE ROYAL INSTITUTION.—In a most able and interesting lecture delivered before the Royal Institution on Friday, February 9, Mr. H. F. Newall, F.R.S., considered the conditions of the Sun's surroundings from two points of view. First, he noticed the many researches that had been carried on of late years into the structure of matter, more intimate than its molecular constitution, briefly touching on the labours of Clerk Maxwell, Nichols, Hull, Arrhenius, Lebedef, Schwarzchild, Poynting, and others. Next, he examined the observations made of the corona during the eclipses of the past decade, in the light which these enquiries had thrown upon them.

Supposing—and there is considerable ground for so supposing—that the neighbourhood of the Sun, far and near, is occupied by meteoric dust of various degrees of magnitude, some of it at least being as finely divided as the matter in comets' tails, Mr. Newall considered the heat that this cosmical dust would experience from the Sun at distances ranging from the actual solar surface to 93 millions of miles at the orbit of the Earth. He showed how, on dust-particles of greater size than 0·0001 of a millimetre, the force of gravitation exceeds that of the pressure of radiation, and the particle is therefore attracted; on particles of a less magnitude the pressure of radiation exceeds that of gravitation, and the particle is repelled; and on particles of excessively smaller magnitude still (whose size is smaller than a wave-length of light, so that they do not cast a shadow, but light presses on all their surfaces) the particles are again attracted. He also pointed out that, as the particles are drawn in or repelled from near the surface of the Sun, they are subjected to rapid and sudden changes of temperature, and such sudden change is the most effective cause of splintering or dividing up particles into smaller parts.

Under the second head of his lecture, Mr. Newall showed several very beautiful coronal photographs, taken by the Astronomer Royal at Sfax, and others by himself at Guelma with a polariscopic telescope. He pointed out the difference in the coronal forms as offered by the two methods employed—direct and polariscopic,—and the evidence thus afforded of the two-fold origin of the coronal light, part being reflected sunlight, and part being emitted by the corona itself.

THE TOTAL ECLIPSE OF THE MOON.—Mr. Denning writes to us from Bristol, under date February 9:—

"This phenomenon was well seen here before sunrise this morning. In the afternoon and early part of night of February 8 extraordinary weather was experienced; there were frequent falls of rain, hail, and snow, with thunder and lightning, and a heavy gale veering to N.N.W. Later on the sky cleared, the ground was covered with snow, and there was a very keen, frosty wind with brilliant moonlight, the conditions being thoroughly typical of a rigorous winter's morning.

"The eclipse seemed to be a decidedly lighter one than usual. From the entering of the Moon into the Earth's shadow until after the total phase was reached frequent observations were made, but the outline of the whole disc, with a large number of included details, continued very plainly visible. At 7 A.M. the bright coppery colour became very striking, and was especially strong on and near the western limb of our satellite. On the opposite side the tone and tint were much deeper and the lunar features feebly visible, this region being involved in the more central part of the shadow-cone, "The Moon became increasingly faint in advancing daylight, and at about $7^h\ 7^m$ A.M. I lost her amid trees and buildings, forming the visible horizon in E. by N. as seen from here."

ROYAL SOCIETY CANDIDATES.—Among the fifteen Candidates selected by the Council to be recommended for election as Fellows are:—Mr. P. H. Cowell, Chief Assistant at the Royal Observatory, Greenwich; Prof. J. H. Jeans, of Trinity College, Cambridge, whose valuable astronomical work is referred to on p. 130 of this number; and Capt. H. G. Lyons, Director of the Helwan Observatory, Egypt. To all of whom we offer congratulations.

PERSONAL NOTES.—Prof. C. W. **PRITCHETT** has retired at the age of eighty-three, after thirty years' service, from the Directorship of the Morrison Observatory at Glasgow, Missouri. He is succeeded by Mr. H. R. **MORGAN**, formerly of the U.S. Naval Observatory. Dr. H. S. **DAVIS** has resigned the position of Astronomer-in-Charge of the International Latitude Observatory at Gaithersburg, and has been succeeded by Dr. F. E. Ross.

Mr. E. T. **WHITTAKER**, M.A., F.R.S., Fellow of Trinity College, Cambridge, has been appointed Andrews Professor in the University of Dublin, and Director of the Dunsink Observatory, a position which carries with it the title of Royal Astronomer of Ireland, in succession to the late Prof. Joly. Mr. Whittaker became a Fellow of the Royal Astronomical Society in 1898 February, and three years later was elected Secretary, which post he still fills.

Mr. **ARTHUR STANLEY EDDINGTON**, B.A., B.Sc. (Manchester), of Trinity College, Cambridge, Senior Wrangler in 1904, has been appointed Chief Assistant in the Royal Observatory, Greenwich.

The death is announced of Dr. S. P. **LANGLEY**, formerly Director of the Allegheny Observatory at Pittsburg, and more recently Secretary of the Smithsonian Institution. Dr. Langley celebrated his 71st birthday last August.

Prof. **KAPTEYN**'s address at the British Association on "Star-streaming" is already in type, and a copy has kindly been sent us by the author. Attention is called to the summary on p. 133.

CORRECTIONS.—On p. 78 of the February number, in Mr. Newall's remark, the words "small star" should be read for "nebula," and the third line from the bottom of p. 79 should read "The size of the image on the slit."

THE next Meeting of the Royal Astronomical Society will be on Friday, March 9; of the British Astronomical Association on Wednesday, March 28.

FROM AN OXFORD NOTE-BOOK.

It is not often that two appointments so important as those of the Royal Astronomers for Scotland and Ireland are made within a few months of each other. Mr. Dyson and Mr. Whittaker are men of proved ability and energy, and British Astronomy may well look forward with confidence to their future administrations. The double appointment will be regarded with satisfaction in Cambridge, and especially in Trinity College, which now numbers the Royal Astronomers of the three kingdoms among its Fellows. It is also satisfactory that the Sheepshanks Astronomical Exhibition (founded in 1858 in memory of the Rev. Richard Sheepshanks) was held in 1888–91 by Mr. Dyson, and in 1894–97 by Mr. Whittaker, and that Mr. Dyson was elected in 1892 to one of the Isaac Newton Studentships founded by the late Dr. F. McClean. Justifications of the Academic record are not so universal that we can afford to pass them by without notice.

THE Edinburgh Chair dates nominally from 1785, when George III. signed a commission creating a Chair of Practical Astronomy, declaredly in the interests of Navigation and Education. The first occupant, Dr. Robert Blair (1786–1828), did nothing beyond drawing his salary of £120 a year, living for some time in London with his son, who was an optician. It is true he had no observatory or instruments provided, but Flamsteed at Greenwich and Bradley at Oxford started under almost precisely similar conditions, and yet managed to do great things. At the time of Blair's death a University Commission was sitting, and recommended that the vacancy be not filled until an observatory could be provided. This was accomplished by the transference to the Professor of the Observatory on Calton Hill, which had been independently erected in 1776 under the auspices of the Edinburgh Town Council. And so in 1834 Henderson was appointed Professor and Director of the Observatory.

HIS all-too-short tenure of office was a brilliant epoch in the history of the Chair, which has been somewhat forgotten. Henderson had been at the Cape for two years, and had worked so hard at observing as to shatter his health. He worked equally hard at the reduction of the observations on taking up his residence at Edinburgh, and his papers, as Prof. Boss declares, are "bright pages in the 'Memoirs of the Royal Astronomical Society.' He was actually the first man to find the parallax of a fixed star (from the observations of α Centauri made at the Cape), but his carefulness in awaiting full confirmation allowed Bessel to anticipate him in publication by a few weeks.

WITH regard to Henderson's appointment, the following paragraph from 'The Story of the University of Edinburgh,' by Sir Alex. Grant, vol. ii. p. 362, is worthy of record. Henderson became a writer's clerk at the age of 15 in Dundee and afterwards in Edinburgh. But he was allowed free access to the Edinburgh Observatory by Prof. Wallace, and had already done good work before his appointment to the Cape in 1832:—

> It is a fact of some little interest that for the vacant post so constituted Thomas Carlyle was a candidate. In his 'Recollections' it is recorded how he thought himself ill-used by his friend Jeffrey, then Lord Advocate, who would not appoint him, but gave the office to a law-clerk instead. Carlyle had shown some aptitude for Mathematics, but it is difficult to believe that such a born man of letters could have so far altered the direction of his mind as to have done any good service in the Observatory. At all events, the appointment of a law-clerk by Jeffrey was very far from being a mere piece of favouritism, as Carlyle seemed to imply.

IT is a striking proof of the estimation in which Henderson was held at the time of his death that the biographical notice of him occupies 22 pages of the 'Monthly Notices' (vi. pp. 158–179). In more recent times, when some have accused us of being unduly prolix in these matters, we find only 17 pages accorded to Airy and 25 to Adams. Henderson's paper on the parallax of α Centauri was read on 1839 January 3, and that on the parallax of Sirius on December 13 of the same year. Fifteen years earlier the Royal Society had awarded the Copley Medal to Brinkley for a supposed determination of parallax, which was not subsequently confirmed.

IT is perhaps invidious to mention this fiasco of Mr. Whittaker's predecessor alongside the triumph of Mr. Dyson's; but Brinkley did quite enough good work independently of his attempted parallax. His determinations of aberration and lunar nutation were for some time the best known. He "spent so much of the latter period of his life in Ireland that he has been considered by some as a native of that country. He was, however, born in England and of English parents, ... he was the senior wrangler of 1788, and was for a short time an Assistant at the Observatory of Greenwich. . . . When Dr. Maskelyne was requested to point out the most qualified person that he knew, as a successor to Ussher, he instantly named Brinkley." We have probably all heard something of the great controversy between Pond and Brinkley (it is curious that they got the Copley Medal in consecutive years), and Brinkley was considered to have the best of it. Recently Mr. Chandler has shown cause for raising considerably the accepted estimate of Pond's work. Now I wonder how this affects Brinkley?

IN the early days of Greenwich Observatory the Astronomer Royal (Airy) fought valiantly against encroaching railways, and managed to protect his instruments from serious tremors by keeping the railway-lines at a distance. He probably thought he had fallen on evil days in being so threatened; but what would he have thought if he could have foreseen all the dangers from electric trains and trams, and light and power stations? When the first electric-train service was started in London the earth-current records at Greenwich were almost ruined. The line was at some distance, and it was several days before it occurred to Mr. Ellis to connect the curious and violent disturbance of the records with the newly-instituted service. To make sure of the facts he visited the line, and found that the time-table corresponded exactly with the hours of perturbation. But now a new trouble arose: on his return from the visit, the *magnets* began to show vagaries. They behaved correctly in his absence, but promptly and invariably resented his presence, until one remarkable day when he appeared at the observatory without any salute from the magnets! I believe it was Mrs. Ellis who found the clue to the puzzle, when asked by her husband what he could have done that day differently from any other day. "Why! you forgot your umbrella," she said; "I found it in the stand after you had gone." And this gave the explanation. The umbrella's spokes had been magnetized on the occasion of the visit to the electric railway, and when it was brought up to the observatory and put in its usual place it disturbed the magnets.

BUT these were early days. Since then electric light has been established at the observatory itself, and methods of "shielding" the dynamos have been studied with success. But the resources of disturbance were by no means exhausted. The London County Council has erected a "power-station" with some enormous chimneys right in the Greenwich meridian (to the north), and it is as yet uncertain what effect this will have on astronomical observations. These recollections and reflections are the result of reading the following paragraph in Prof. E. C. Pickering's 60th Annual Report (September 30, 1905), p. 6:—

> Measures of the photographic brightness of the sky, due to electric lights, have been made at Ossipee, North Lexington, Concord, Waverley, Arlington Heights, Cambridge, and Boston Common, with results which may be expressed by the numbers 23, 33, 36, 50, 52, 100, and 242 respectively. The fact that the sky at Cambridge is three times as bright as at points only a few miles distant, suggests that it may be necessary in the future to establish an auxiliary station for the northern stars.

THIS particular paragraph is picked out from the Report because it more or less concerns us all. As regards the rest, it reads like the doings of astronomers in another world. How are we to enter into the feelings of people who discover 8 new stars and 1129 new variables, as well as new satellites of Saturn? We can only gasp. And the Director explains that if he only had a little more money, he could really do some work!

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EDITED BY

T. LEWIS, Sec.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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Vol. XXIX. APRIL, 1906. No. 369.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1906 March 9.

Mr. W. H. Maw, *President*, in the Chair.

Secretaries: E. T. Whittaker, M.A., F.R.S., Andrews Professor, and T. Lewis.

The Minutes of the last Ordinary Meeting were read and confirmed.

Mr. Lewis. One hundred and one presents have been received since the last Ordinary Meeting, including amongst others the following books:—'Modern Cosmogonies,' by Miss A. M. Clerke, presented by the author; new editions of 'Remarkable Comets' and 'Remarkable Eclipses,' presented by the author, Mr. W. T. Lynn; Paris Observatory Memoirs, vol. 24 (Gaillot, "Tables de Saturne"), presented by the Observatory; Philadelphia Observatory Publications, vol. ii. parts 2 and 3 ("Measures of Double Stars"), presented by the Observatory; and 'Tokyo Observatory Annals,' vol. iii. parts 3 and 4. We have also received from the Royal Observatory, Greenwich, eight original negatives of the Sun, taken 1901 October to November, and 36 enlargements of the Astrographic Chart; Photographs of the Solar Eclipse, 1905 August 30, presented by Mr. E. W. Barlow; and two photographs of the flash-spectrum, taken on the same occasion, and presented by Dr. S. A. Mitchell.

The President mentioned the names of three defaulters who were no longer Fellows of the Society, and then called on Mr. Cowell to give an account of two papers presented by him.

Mr. Cowell. These papers are simply a determination of the constants of elliptic motion for Venus and the Earth, and the mass of Venus is also determined. The material used goes down to the year 1900, when Le Verrier's Tables ceased to be used in the *Nautical Almanac.* The results scarcely differ from results already obtained by Prof. Newcomb from material ending with the year 1892. I therefore do not wish to dwell upon the results, but it is about the method I wish to speak. It has been usual to form from each small group of about half a dozen observations an equation of condition containing several unknown quantities, and to solve a system of many hundred such equations by least squares. The reader has to take the results of such an investigation upon trust. The subject is, so to speak, thrown into the computer's melting-pot, and the results finally appear after a process which the reader has not time to reproduce and cannot clearly follow. The peculiar feature of my method is the absence of simultaneous equations. A necessary consequence is that the relations between the various unknown quantities distinctly appear, and the calculations of the paper could be repeated in a very few hours. The simplification was achieved by forcing symmetry upon the observations. Where observations were isolated great weight was attached to them; where observations were crowded, the weight of each was diminished. In fact the weight assigned to observations in any interval was proportional to the interval. It is shown in the paper that any loss of accuracy incurred through so artificial a system of weighting is more than counterbalanced by the importance of ascertaining the relations between the unknown quantities.

Prof. Turner. The point raised by Mr. Cowell is a very interesting one. I have only had an opportunity of seeing the paper for a few minutes, and I do not know whether I quite rightly understood it. If I have, it seems to me to be analogous to an interesting question raised in another department of astronomy by Mr. Schlesinger. When I was in America two years ago Mr. Schlesinger raised the following point which, whether analogous or not to Mr. Cowell's point, is of interest. Suppose you are measuring a star for parallax, and that there are four stars, A, B, C, and D, immediately surrounding it at exactly equal distance, so that the star is at the centre of gravity of B, D, or of A, C. Then in applying a formula of reduction, $ax+by+c$, from A, B, C, D, it is only necessary to take account of the constant term c. Now in practice you cannot get four stars symmetrically distributed like that, but one of them, say C, is sure to be at some other distance, let us say, for instance, twice the distance: you can get the same effect if you give *half* the weight to the observation of C; measure it on the plate and give half weight, and that is just the same as though you had the star half way. I must confess that when this procedure was mentioned to me by Mr. Schlesinger it took me aback at first, but he defended it in the following way:—He said there is no need to measure star C *at all*, and that is like giving no weight at all. And you might measure and give it full weight. Why not, then, adopt an intermediate procedure and give half weight? On the whole I was inclined to agree that this procedure was allowable. It has exactly the same merit in his case as in that of Mr. Cowell; it avoids the necessity of a lot of numerical work. This principle of weighting observations in an arbitrary way so as to avoid a lot of numerical work does seem of great interest and value, which, perhaps, is capable of extension into many departments of astronomical work.

Mr. Conrady. This note that I offer was suggested by Prof. Turner on the subject of some dark slides that were taken to Egypt, and helped to spoil some photographs by fogging the negatives. Prof. Turner thought that it would be interesting to have every possible information as to those particular dark slides, and so I have made enquires. But I am afraid the result is largely negative. The log from which the slides were made must have been imported well over 15 years ago, because about that time it was cut into boards for seasoning purposes and has been seasoning ever since, first at the timber merchant's, and for the last nine years in the place of the makers of the slides. Moreover, the slides were not new ones. They were made 2½ years ago and had been in stock since; and one would think that if anything volatile had got into them in the course of manufacture that would have had time to evaporate. So there does not seem any accounting for their behaviour, especially as hundreds of other dark-slides have been made from the same log of Spanish mahogany. It is interesting that when these slides were returned the experiment was made of charging them with plates here, and putting them into an ordinary English cool dark room instead of one exposed to the dry heat of Egypt; and it turned out that in a week's time these slides would fog plates even here. But at my suggestion a strip of tin-foil was put under part of each plate; and that completely prevented the action, so that whatever may be the cause of fogging it cannot penetrate tin-foil. Unfortunately this otherwise valuable information has proved unavailing, because it has been found impossible to attach tin-foil to wood in such a way that the slide would stand pulling in and out. Tin-foil being impervious to moisture causes the glue—which is the general resort of cabinet makers—to set into a jelly which cannot dry. Anyhow, a piece of tin-foil attached to mahogany by glue could after a fortnight be stripped off without the slightest difficulty. But it was further found that the penetrating power of the emanation, or whatever it may be, is so slight that an extra heavy coating of the ordinary optical black—which is simply ordinary shellac-varnish mixed with lamp-black—absolutely stopped the effect; and that was how the slides have been set right. It is rather interesting that apparently the fogging effect was limited to two out of three slides; at any rate the third one has not been returned.

Prof. Turner. The same fogging effect was caused by the third slide, but I only sent two of them to the makers.

Mr. Conrady continuing said:—The question is, seeing that no other set of slides made from the same wood has ever shown any defect of this kind, whether exposure to extreme heat had caused some change in the wood, setting free some volatile matter in it and thus causing the effect. I am afraid that will be very difficult to ascertain; and I can only repeat the suggestion made by our President at a previous Meeting, that for work which cannot be repeated the slides should be tested before being taken out under as nearly as possible the same conditions as it is proposed to use them. For instance, I suppose the climate of Egypt might be sufficiently nearly imitated in a boiler-house; and there are plenty of these to be had nowadays, say at the Central Electric Lighting Stations. We do not know anything as to the cause of the defect in these dark slides, though I believe one gentleman has been working at it for a number of years and yet is still in the dark. The trouble seems to turn up occasionally in other woods, especially in pine. But in mahogany it seems to be a very rare occurrence.

The President. Perhaps a coating of bronze powder, the metallic particles of which would soak in, might be effective.

Mr. Conrady pointed out that even a coating of plain gelatine over the sensitive film would stop the fogging.

Mr. Bryant asked whether the shellac-varnish was tried in a dry heat corresponding to that of Egypt.

Mr. Conrady. It would notoriously stand practically anything without cracking.

Prof. Turner, who was asked whether he had any comment to make, said:—Nothing, except that a preliminary test may not be sufficient. They would need to be tested twice over. I think Mr. Conrady's remark significant that Dr. Russell has been at work on this subject some time without getting any light on it. It seems that a considerable number of experiments will have to be made before we get at the complete facts.

The President, Has the matter been brought before the Royal Photographic Society?

Mr. Conrady. I believe Mr. Watson himself is a member of that Society and thus could bring it before them if he wished. If you like I will suggest it to him.

Mr. Inwards. Something more than shellac might be considered—for instance, the substance used by French polishers to fill up the pores of the wood before they proceed with their work.

Mr. Thackeray (presenting a paper on the comparison of two star-catalogues). A comparison has been made in this paper between Carrington's well-known circumpolar catalogue for 1855 and the Greenwich Groombridge system depending on the new reduction of Groombridge. The results in Right Ascension call for no particular mention, those in N.P.D. show discordances which are most probably due to the division errors not having been determined by Carrington. In general the results point to the care and accuracy with which Carrington compiled his catalogue.

A diagram was thrown on the screen showing the differences between the R.A. and N.P.D. of the two catalogues, arranged in zones for each degree of declination.

Mr. Franklin-Adams. I do not like to say for certain, but I believe that I have the circle with which these observations of Carrington were made.

Dr. Rambaut. No, Sir! I have it.

Mr. Franklin-Adams. I am glad that I expressed a doubt about the matter before saying more, but it is better for the point I was about to bring forward if Dr. Rambaut has it and not I. I was going to say, why not examine the division errors?

Dr. Rambaut. I am disconcerted that Mr. Franklin-Adams should think he has the Transit Circle used by Mr. Carrington. We have been under the impression that that was the one with which the Radcliffe Catalogue of 6000 stars was made. The circle of the Oxford instrument is a particularly well-divided one, in my opinion. Carrington seems to have given a great deal of attention to the questions of divisions, and the divisions are cut in a gold band, which I believe is not a usual thing; and the lines seem to be particularly clean. When I came to Oxford I was struck with the accuracy with which one could set upon those lines. With regard to the comparisons which Mr. Thackeray has shown us, I was intending to ask him whether he had not applied the corrections for these division errors which were found by Mr. Stone in the years 1881 and 1882. There were a very large number of observations made in order to get these, and they were made in a rather unusual manner. I have quite recently been re-examining these observations, and it seems to me that they hang together very well, for the observations are very consistent, and the corrections as deduced by Mr. Stone are worthy of a great deal of confidence. I hope at some near date in the future to make a further re-examination of the question; and, perhaps, push the observations so as to get the division errors complete, for I should say that Mr. Stone has only finished the alternative degree lines.

Mr. Lynn. I well remember Mr. Main recommending the Trustees to buy the Circle; and a short time after they adopted his advice.

Mr. Thackeray. The only thing I can say is that I had no idea the telescope was in existence. We shall be very happy indeed to have the corrections if Dr. Rambaut can supply us with them. The difficulty, however, will be to identify the position of the circle, as it was movable round the axis.

The President in according thanks to Mr. Thackeray, added that if further researches were made they would be glad to learn the results.

Prof. Whittaker gave the substance of *Dr. Downing's* paper on the Total Solar Eclipse of 1908, January 3, which, it appears, will be visible from two islands in the South Pacific—Flint Island and

Hull Island. Dr. Downing communicates the maps of these islands. One of them appears to be a coral-reef enclosing a lagoon; and the other is also fringed by a coral-reef two miles long. Neither island appears to be inhabited at present. Landing is said to be extremely difficult. The rest of the data relates to the eclipse, and can be more easily gathered when the paper is printed. The duration of totality will be three minutes at Hull Island and four minutes at Flint Island. The altitude of the Sun will be 43° at one and 47° at the other.

Mr. Crommelin. Flint Island, if I remember aright, was also the scene of an Eclipse Expedition in 1883 *. The neighbouring Caroline I. was occupied by a large American party; and I think in the report it says that one or two observers went over to this island to avoid the risk of failure through clouds. At Caroline Island they had some difficulty in landing; the ship could not come near the shore, and their luggage had to be carried through the water by sailors wading up to their waist. There seems, therefore, to be a precedent for occupying the island on this occasion.

Prof. Turner. Is it true that these islands are now in the possession of the Sunlight Soap Company?

Prof. Whittaker. I understand that they had been taken over by Lever's Pacific Plantation Company!

Mr. Evershed read a paper describing the spectroscopic outfit employed in the Solar Eclipse of 1905, August 30, by *Dr. S. A. Mitchell*, of the U.S. Naval Eclipse Expedition. Dr. Mitchell was stationed at Davoca, in Spain, and was fortunate in having good weather on the day of the eclipse. His principal instrument was a large grating ruled on a parabolic concave surface and used without slit or lenses. Flat gratings and prismatic spectrographs were also used. The programme of work included photographs of the flash-spectrum in the less refrangible region as well as in the ultra-violet, and photographs of the corona-spectrum. An attempt was also made to estimate the intensity of the continuous spectrum of the corona at different wave-lengths.

A good photograph of the corona-spectrum was secured with a flat grating and lens, and this shows a good many coronal rings in the ultra-violet as well as in the less refrangible part of the spectrum. Those in the extreme ultra-violet Dr. Mitchell estimates have as great an intensity as the green line at λ 5303.

With the parabolic grating a very beautiful photograph of the flash-spectrum was secured at third contact; this extends from D‴ to λ 3300, and is in excellent focus throughout. In the green region the definition is particularly fine, and among the interesting features shown are the two pairs of lines at λλ 4922–24 and 5016–18. In each pair the more refrangible component is a helium line and the less refrangible component is an iron spark line; in the chromosphere spectrum, just outside the band of flash-lines, the components of each pair are of about equal intensity; but in the flash-region itself the helium lines become almost invisible, whilst the iron lines are very much enhanced. These lines are amongst those constantly seen in metallic prominences, and the relative intensities of the components vary according to the extent to which the flash-region is elevated into the chromosphere.

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* Subsequent investigation shows that this occupation of Flint Island in 1883 was suggested but not carried out.

The helium-spectrum, unlike hydrogen, with which it is so closely associated in the chromosphere, seems to decrease in intensity in a very marked manner towards the base of the chromosphere; in this respect it resembles the unknown gas giving a line at λ 4686, which itself would seem to form a connecting-link between the chromosphere and corona.

The majority of the fainter arcs in Dr. Mitchell's photograph extend over about 12 degrees of the Moon's limb, indicating a depth of radiating gases of only ·28″; and as the Moon's motion relative to Sun was ·40″ per second, it is easy to realize what a short time is available for photographing this spectrum. Dr. Mitchell is to be congratulated on having timed this photograph so well, and upon having such a good focus over the whole range of spectrum.

Another photograph obtained immediately after third contact shows a narrow band of continuous spectrum upon which the Fraunhofer lines are clearly defined, although very much reduced in intensity. It is a significant fact that within 1″ or 2″ of the Sun's limb the intensity of all the dark lines falls off very rapidly, although apparently varying to some extent at different points on the limb.

Mr. Newall. It is extremely interesting to see the splendid results that Dr. Mitchell seems to have got, and we are greatly indebted to Mr. Evershed for the clear account he has given to us of the pictures. Those pairs of lines he mentioned in the green part of the spectrum are of peculiar interest. But I feel I ought not to refer to them, because I hope the Astronomer Royal for Scotland will do so. I was fortunate enough to get a photograph of the flash-spectrum at Guelma with a large grating spectroscope without any slit, and that is on an even larger scale, though showing a smaller range of spectrum than those shown by Dr. Mitchell. But I am in the happy or unhappy position of having got so many records at Guelma that I have not yet been able to study them all adequately; but I look forward to tackling these spectra with great interest, because these special lines in the green region are of obvious importance, and should be recorded on my photographs.

Prof. Dyson. I would like to offer my congratulations to Dr. Mitchell on obtaining the beautiful spectra we have seen. At the last eclipse I was fortunate enough to arrange my slit so as to cut through the large prominence which appeared on the Sun, and with an exposure from 20 seconds after the beginning of totality to 20 seconds from the end the spectrum was obtained of the prominence at some distance from the Sun's limb. Two lines to which Mr. Evershed referred appeared strongly marked—the helium line and the spark iron line. In comparing them with photographs I had obtained previously at Sumatra it is seen that in those photographs the helium line is not shown, or is shown extremely faintly if at all; but the spark iron line is well shown, confirming what was said by Mr. Evershed, that the helium line is stronger at some distance from the Sun's limb. It seems to occupy a curious position, because it does not occur in the corona, but just occurs in the highest parts of the chromosphere.

Mr. Fowler. I am particularly glad to hear that Dr. Mitchell has been so successful in photographing the less-refrangible parts of the flash-spectrum. It is here that most of our ordinary observations of the spectra of sun-spots and prominences are made, and we have long felt the need of better eclipse data for comparative study. I believe that we have many opportunities of observing the flash-spectrum besides those we get during eclipses. I have made many observations of disturbed parts of the chromosphere, and, so far as it has been possible to make the comparison, it appears that we get the same lines as in the flash-spectrum. It would seem that when the chromosphere is sufficiently disturbed, the flash-stratum is lighted up into view, but a more minute comparison is necessary to completely establish this relation. Some of the peculiarities of sun-spots will probably also be better understood when we have a more precise knowledge of the chromospheric and flash spectra. I wish to thank Dr. Mitchell and Mr. Evershed for giving us the opportunity of seeing this very fine series of photographs.

Dr. Lockyer. I think Mr. Evershed said that Dr. Mitchell had photographed the coronal rings, and that the violet ring was probably more brilliant than those in the blue and green parts of the spectrum. In the photograph we secured during the last eclipse, the green ring is undoubtedly strongest of all the rings in the spectrum obtained with a 6-inch 3-prism prismatic camera, although the sentitiveness of the photographic plate used was very much greater in the violet region; if the violet ring was really strongest, then it should be the most intense in the photograph. The photographs we obtained last year corroborate those taken on former occasions at eclipses, on which I think we found the green ring was always seen as the brighest.

Prof. Dyson. I observed the lines in 1901, and they were fairly strong; but I agree with Dr. Lockyer that they are not so strong as the green line.

Mr. Conrady. The flint glass used in the prismatic camera greatly absorbs ultra-violet light, whilst the grating camera would give the ultra-violet light even more strongly than the green. The differences seem to be in that direction.

The Astronomer Royal. There is another point. The irrationality of dispersion would affect the question very much. Of course, with the prismatic camera the ultra-violet light is spread out very much more.

Mr. Newall. Does Dr. Mitchell give the wave-length of the ultra-violet line? There is an interesting line in the spectra of nebulæ; I mean the line discovered by Sir W. Huggins, and utilized by Dr. Hartmann in his ultra-violet photographs of the Orion nebula. It would be interesting to know whether there is any connection between these two luminosities.

Mr. Evershed. The wave-lengths of the coronal rings are given in the paper; the ring referred to by Dr. Mitchell as being of the same intensity as the green ring is at λ 3388. At the eclipse of 1898 I took a photograph showing this ultra-violet ring at the extreme end of the spectrum, and I should say that it had the same *form* as the green ring, but I could not say whether the intensity was greater or less.

In estimating the intensity of a coronal ring or line, one judges by the intensity in relation to the continuous spectrum. When glass prisms are used the intensity of course falls off rapidly in the ultra-violet, but this affects equally the ring and the continuous spectrum.

In reply to Mr. Newall I have pointed out to Prof. Campbell the apparent coincidence of the corona lines at λλ 3388 and 3456 with lines discovered by him in Nova Persei after it had assumed a nebular spectrum; but he considered the evidence for the wave-lengths in the star's spectrum not good enough to found any conclusions upon. I do not think that the nebula line discovered by Sir W. Huggins at 3727 is represented in these spectra.

Prof. Whittaker gave a summary of the paper presented by *Dr. Alex. Roberts* on the radial velocities of spectroscopic binaries which are also variable stars. There can be little doubt that the revolution and variation are in some way connected with their joint working as binary systems, and by way of attacking this problem he suggests that the spectroscopic determinations of radial velocity should be investigated rather thoroughly. His starting-point of investigation is the measurement made by Prof. Campbell in 1901. Prof. Campbell observed that the velocity-curve in this case contains evidence of a secondary velocity-curve having half the period. Dr. Roberts proposes to account for the existence of this secondary term by supposing that two masses are rotating round each other and very much disturbed by their mutual attraction.

Mr. Newall. I should like to read this paper in print before passing an opinion upon it. It strikes me as a difficulty that Dr. Roberts seems to pick out the light from particular parts of his elongated bodies. With the spectroscope one has got to deal with the integrated light from the whole exposed surface of the body, and that would result in widening of the lines in addition to their periodic shifting. I do not think that I have understood Dr. Roberts rightly.

The President. When the Gold Medal was awarded to Prof. Campbell I asked whether he would send over some slides of the spectra taken by him for exhibition at our Annual General Meeting. He very kindly acceded to this request; but owing to rough weather in the Atlantic the mail which brought the slides over was delayed for three days, and instead of getting them in time for the February meeting they did not arrive till the Saturday after. I have got them to-night and will now show them.

The Astronomer Royal then took the Chair.

The following photographs sent by *Prof. Campbell* were then thrown on the screen and described by *Mr. Maw*:—The Mills Spectroscope; Spectrograph of Polaris taken 1899 November 19, showing a radial velocity of −18·7 k. per sec., which, when corrected for the Earth's velocity, gives −13·4 kilometres per sec. for the velocity of the star; Spectrograph of *ο* Ceti, 1898 August 29; Spectrograph of *ι* Pegasi, consisting of a spectrum taken on 1899 October 17, showing a velocity of 35·95 kils. per sec. receding, and another taken 1899 October 24, showing a velocity of 42·54 kils. approaching; Spectra of Nova Persei taken at different dates; Photograph of a drawing made by Prof. Campbell showing the bright lines in the spectrum of Nova Aurigæ, showing (Prof. Campbell considers) that the star had been transformed into a nebula.

Pictures were also shown of the 37-inch Reflecting Telescope for the Mills Expedition to the Southern Hemisphere and the station of this expedition near Santiago, Chile. At this station, which is munificently supported by Mr. D. O. Mills, the radial volocities of nearly 200 stars in the Southern Hemisphere have been made, and the work is being extended to fainter stars.

The Astronomer Royal mentioned that papers comprising photographic observations of comets for the years 1903, 1904 were presented to the Society from the Royal Observatory, Greenwich, that the work of measurement and reduction of several such comets was in hand, and that these arrears would shortly be cleared off.

The following papers were announced and partly read:—

S. A. Mitchell. "Preliminary Account of Flash-Spectra taken 1905, August 30."

Alex. W. Roberts. "Notes on certain Anomalies observed in Radial Velocity Curves."

A. M. W. Downing. "The Total Solar Eclipse of 1908, January 3."

A. Stanley Williams. "On the Variable Star RX Andromedæ."

P. H. Cowell. "Discussion of the Greenwich Observations of the Sun, 1864–1900."

P. H. Cowell. "Discussion of the Greenwich Observations of Venus, 1869–1900."

W. G. Thackeray. "Comparison of Carrington's Circumpolar Catalogue with the Greenwich-Groombridge System."

Royal Observatory, Greenwich. "Observations of Comet *c* 1903, from Photographs taken with the 30-inch Reflector of the Thompson Equatorial."

Royal Observatory, Greenwich. "Observations of Comet *e* 1904, from Photographs taken with the 30-inch Reflector of the Thompson Equatorial."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon made in the Year 1905."

A. E. Conrady. "Note on Mr. Reynolds's Dark Slides."

The following gentlemen were elected Fellows of the Society:—

***Frederick Joseph William Crowe,* Marsden, Chichester.**

***Thomas Edward Heath,* 53 Park Place, Cardiff.**

***Samuel Thomas Johnson,* Melrose, Bridport, Dorset.**

***George Tyrrell MacCaw,* Geodetic Survey, North-eastern Rhodesia, South Africa.**

***Percy Alfred Talbot,* B.A., F.R.G.S., F.A.I., Royal Societies' Club, St. James's Street, S.W.**

The following Candidates were proposed for election as Fellows of the Society:—

Edmund Dickson, F.G.S., 2 Starkie Street, Preston, Lancashire (proposed by A. L. Cortie).

Jacob Karl Ernst Halm, Ph.D., Royal Observatory, Edinburgh (proposed by F. W. Dyson).

John William Hicks, Computer to the Ordnance Committee, Ordnance Committee Office, Woolwich (proposed by Sir W. H. M. Christie).

William Malin Hunt, Engineer, 48–50 London Road, Nottingham (proposed by Samuel Fellows).

Lieut. Edwin Baikie Simpson-Baikie, R.N.R., 142 A, The Bluff, Yokohama, Japan (proposed by D. P. Richards).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1906 February 28.

A. C. D. Crommelin, *President*, in the Chair.

Secretaries: J. A. Hardcastle.
J. G. Petrie.

Mr. Petrie read the Minutes of the previous Meeting, which were confirmed and signed.

Among the presents announced the *President* drew attention to the gift by Miss Alice Everett, M.A., of Parts 1 and 3 of the 'Photographic Catalogue of the Heavens' (Potsdam Observatory), and also to a small photographic telescope, given by Mr. A. E. Mitchell, of Dublin. The thanks of the Meeting were unanimously accorded to the donors.

The names of seven Candidates for Membership were read and passed for suspension, and the election by the Council of three new Members was unanimously confirmed.

Mr. Scriven Bolton contributed a paper on "Fluctuations in the Velocity of Jupiter's Great S. Tropical Marking, 1901 to 1905," accompanied by a drawing showing the S. tropical disturbance and a chart representing the variations in the rotation period. The paper was read by the *Rev. T. E. R. Phillips*, the Director of the Jupiter Section. He remarked that, although some of them might be inclined to question some of the details that Mr. Bolton reported on Jupiter, yet they would all appreciate very highly his remarkable assiduity and his great perseverance and diligence and would doubtless all admit that his draughtsmanship was absolutely beyond praise.

Mr. Hardcastle asked if the Red Spot was visible now.

Rev. T. E. R. Phillips said it was, but it was very faint and difficult to see except under good atmospheric conditions. Mr. Scriven Bolton saw it with a 4-in. telescope, but most people would doubtless find it very difficult to detect it with so small an aperture. Mr. Phillips added that he had received one or two other communications with reference to another matter discussed at the January Meeting, viz., the illusion of the flattening of the limb when one of the satellites was in close proximity to it. It was strange that good and experienced observers should have this illusion, and that at other times, when they specially looked for it, they could not see it at all.

The Rev. R. Killip said that on a recent night he observed the disappearance, by occultation, of Satellite No. 1, and the peculiar flattening appearance was quite marked for some five minutes before the contact. But the very moment the satellite touched the limb, the thing was proved at once, he thought, to be an illusion. It did not recur. Mr. Killip added that some two or three months ago he had seen Satellite No. 1 for fully two minutes after it was well behind the disc by occultation—that was to say, he supposed that through the spurious limb of Jupiter, due entirely to interference effects, the satellite was visible. He thought that similar anomalous appearances had been frequently referred to, especially in Webb's 'Celestial Objects.'

The President was very glad to see such evidence of assiduous work on the part of the Jupiter Section. Certainly the curious movements of these markings on Jupiter gave plenty of occasion for careful study. It was difficult to see much law in the erratic drifting of this black marking in the S. tropical region, and certainly it was very desirable to watch its passage across the Red Spot to see if it behaved in the same curious way as it did in the previous passage to which Mr. Phillips had referred. He could wish that the observers of Saturn could do some of this same work, viz., taking the times of transit of different markings.

Mr. G. J. Newbegin gave a paper entitled "Remarks on the Solar Prominences observed in 1905," illustrated by slides, which were thrown upon the screen. During the year 48 observations were made between February 16 and October 18 inclusive, each observation having yielded some result. Two hundred and twenty-four bright and two hundred and forty-five faint prominences were observed and noted down. On three occasions black matter was observed, viz., May 16, June 21, and July 7; that of May 16 had the appearance of a black bar cutting the prominence at an angle of 45°; on June 21 the black dot was at the apex of the prominence, just beneath the bright matter. On July 7 the black matter was at the base of the prominence in three places. Reversals by sun-spots occurred on February 28, March 9, May 8, October 5, 17, and 18. The complete observations had been forwarded to the Director of the Solar Section for his use in the preparation of the "Solar Memoir" for 1905.

Mr. Maunder said he was interested to hear that Mr. Newbegin had again recorded several dark markings on the prominences. He would like to know whether the appearance was that of dark absorbing matter between the prominences and ourselves, or whether it was merely a gap in the form of the prominence.

Mr. Newbegin said he was inclined to think there was black matter, but it was rather dangerous to attempt to give any reason for these appearances.

The President was sure all would feel that Mr. Newbegin deserved great credit for the assiduity with which he carried on these prominence observations. There were very few Members of the Association who did this work; indeed it was little done in England at all, and it was certainly very valuable, adding a great deal to our knowledge of solar physics. It looked as if the prominences followed the spots in getting nearer the equator as the maximum approached. That, he thought, was the same law as the spots would follow.

Mr. Maunder. Just the same.

The President. So there seemed to be some very intimate connection between the prominences and the spots.

Mr. Alexander D. Ross contributed a paper on "The Fireball of 1905, December 30," which was read by *Mr. Hardcastle.* Since communicating a previous note the writer had collected as many accounts as possible of the phenomenon, and the reports included the observations of 50 individuals. From the number of reports received a fair amount of reliable information could be collected, and from this a determination was made of the real path. The fireball first became visible at a height of 69 miles over a point 8 miles west of Thornhill in Dumfriesshire. It pursued a path of 64 miles in length with a velocity of about 15 miles per second, and finally disappeared at a height of 31 miles above a point in the sea 7 miles south of Arran. This gave an earth point 8 miles north of Rathlin Island, and a radiant at R.A. 43°, Dec. 26° N. Mr. Denning had deduced a real path from several reports sent to him, which, considering the nature of the data, was in very good agreement. The writer then proceeded to estimate the mass of the meteor from the available data, and concluded that it would be something of the *order* of a few ounces or a few pounds.

Mr. Hardcastle said he had a communication sent him from Derbyshire regarding this meteor, and although it must have been 150 miles away, the observer described the trail he saw. It was certainly very remarkable that the trail should be visible at such a distance.

Mr. G. M. Seabroke said it had to be recollected that the eye was the worst photometer one could imagine. The light might be accounted for, but the difficulty was the length of time that the trail was seen afterwards.

The President remarked that the meteor under notice had certainly given rise to two or three points of interest. One was the determination of the mass of the meteor, and it was startling to find that such a tiny body, a body that one could easily hold in one hand, according to the estimate made by Mr. Ross, could make such a wonderful display over a great part of the British Isles. The fact that meteors were of such small dimensions showed that the immense estimates of apparent size that were often made by observers—"looking larger than the Moon," for instance—must be entirely fallacious, and only the result of optical illusion; because, as a rule, the meteor was several miles away from the observer, and of course a few feet in diameter would look a mere point at that distance. The trails, of which pictures had been shown, were of great interest to watch, because they gave us one of the very few ways we had of seeing how the currents were moving in the upper air.

Mr. Hardcastle read a paper on "The Firmament," by *Mr. S. Fellows.*

The President considered that Mr. Fellows's paper put in a very attractive way the gradual growth of knowledge of the reality of the heavens as opposed to their appearance.

Mr. R. Leetham Jones exhibited a photograph of the Eclipse of the Moon on February 9 last, which was thrown upon the screen. He said it might be of interest to some members as showing what could be done with no other apparatus than an ordinary half-plate portrait camera.

A paper on "Ancient 'Eclipses," by *Mr. W. H. S. Monck* was read by *Mr. W. T. Lynn.* The main point of the paper, said Mr. Lynn, seemed to be to call in question nearly all the ancient accounts of eclipses with what he might call a spirit of hyper-scepticism. They must in every case consider the whole account on its merits when comparing it with results obtained by calculations. He was glad to find that Mr. Cowell had come to the conclusion that, in several cases, at any rate, they could really identify ancient eclipses with those resulting from the calculations of modern astronomers, and very important results had been derived from some of these. Although Prof. Newcomb was hyper-sceptical, like Mr. Monck, he (Mr. Lynn) ventured to adhere to the other view, and contended that, in some cases, certainly we could positively identify ancient accounts with the modern calculation of eclipses.

Mr. Hardcastle read a report of the Rousdon Observatory, communicated by *Mr. C. Grover.* The report was devoted mainly to observations of about 25 Long-period Variable Stars, mostly circumpolar, during the year 1905.

The President said it was satisfactory to find that, in spite of the death of Sir Cuthbert Peek, work was still being pushed forward energetically at Rousdon Observatory.

Notes from *Capt. Daunt* and the *Rev. J. T. Bird* on "The Zodiacal Light" were next read.

Mr. Hutt said he was surprised to find the appearance of the Zodiacal Light treated as a wonder. Some 16 to 20 years ago he used to see it regularly at Palmer's Green almost every clear evening about the end of January and the beginning of February.

Miss Stevens quite agreed that it might be seen on any clear night after sunset at this time of the year.

The President urged that all observers of the Zodiacal Light should make notes of its shape, extent, and position.

The Meeting then adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

At the Meeting of this Society held on Wednesday, March 21, at the Institution of Civil Engineers, Great George Street, Westminster, the President, *Mr. R. Bentley*, in the Chair, *Dr. H. R. Mill* gave an interesting lecture on "South Africa as seen by a Meteorologist." This was illustrated by a series of latern-slides from photographs taken during the tour of the British Association in 1905. The places visited included Cape Town, Table Mountain, Durban, Maritzburg, Ladysmith, Johannesburg, Pretoria, Bloemfontein, Kimberley, Bulawayo, the Matoppo Hills, the Victoria Falls of the Zambezi, Salisbury, Umtali, and Beira. During the return journey, Mombasa, Cairo, and the Seuz Canal were visited.

Photographs were shown of meteorological stations in many of the places named, and the views of the scenery were selected to bring out the climatic features.

Changes in Stellar Spectra.

Enquirers into the physical condition of the stars need to keep their minds on the alert and their convictions pliable. Fresh information bearing on the subject is continually being provided; and it should be assimilated, not merely pigeon-holed. This, indeed, is a counsel of perfection most difficult to follow. Facts are always hard, and often angular or misshapen, and they are apt to fit in imperfectly with those already arranged into the mosaic of our thoughts. They may even, by their recalcitrance, compel its total break-up and reconstruction.

None are more unmanageable than those connected with variations in stellar spectra. A considerable body of them are at hand; they have been securely ascertained; they are of divers kinds, and their implications are frequently unlooked-for and far-reaching. Some appear scarcely reconcilable with current ideas regarding the physical history of the stars. For they indicate abrupt transformations of spectral type manifesting themselves as unrelated and isolated phenomena, in marked contrast with the gradual, almost insensible progression through cooling, postulated as the groundwork and rationalizing principle of modern theories of stellar evolution.

Nearly all stars that vary extensively in long periods show concurrent changes in the quality of their light. This is only what might have been expected. It is quite intelligible, if not inevitable, that extreme vicissitudes in total brightness should be accompanied by waxing and waning symptoms of gaseous incandescence. These symptoms, it is true, when regarded in detail, present many features of perplexity; but we cannot now stop to discuss them. Our concern is rather with certain abnormal instances of spectral change, indirectly, if at all, dependent upon the ordinary processes of luminous variation.

One of the most striking is met with in R Cygni. This star oscillates from the sixth to about the fourteenth magnitude in a period averaging 426 days. Its spectrum has been frequently observed, and for the most part, owing to lack of light at other times, near epochs of maximum. Dunér found it, in 1880 and 1883, to be of the third type, finely developed; and Mr. Espin was astonished to perceive on the 13th of August, 1888 (the phenomenon being then a rarity), a vivid green line of hydrogen emblazoning the dusky bands [*]. On October 1 following, the star having sunk nearly to the ninth magnitude, Mr. Maunder noted the spectrum as generally of the fourth type, though Hβ continued bright [†]. Two years later, however, it was shown by a Harvard College photograph, taken at culminating brightness, to have reverted to the Mira pattern, formed as usual of titanium-flutings relieved by emission-lines of hydrogen. Finally, a spectrogram obtained with the same instrument, December 7, 1904, fourteen days before maximum, recorded the fourth-type spectrum of a carbon star, devoid of any extra incandescence. The possibility of alternations in stellar types was thus clearly demonstrated. The same object can, it seems (though doubtless under exceptional circumstances), migrate from one to another, and eventually resume its primitive aspect. Whether R Cygni, as it emerges from the obscurity of its minima, will be characterized by titanium or by carbon-absorption, lies beyond the reach of prediction. But the very indeterminateness of the outcome is in itself of great significance. We learn from it unmistakably that the lines of evolution pursued by the two varieties of stars with banded spectra do not strike widely apart, and are not always irrevocably chosen. Communications are maintained between them; flittings across the border are feasible; the distinction between members of the diverse types may at any moment be effaced by unusual disturbance. As regards R Cygni, indeed, the inference forcibly suggests itself to our minds that the photospheric cloud-shell is actually dissolved and reconstructed in the course of each cycle of light-change, the new photosphere being often differently composed, and differently situated within the vaporous globe, from its predecessor. If this be so, the spectral alternations of the star would cease to be wholly perplexing, since they could be related, with a high degree of probability, to varying modes of photospheric condensation [‡]. The signs of a close affinity between members of the third and fourth types, detected by Prof. Hale as one result of his photographic investigations, are then strongly accentuated in R Cygni. Emphatically, in Bacon's sense, a "travelling instance," the variable exhibits this or that "nature" through the compulsion of forces, the character of which, now profoundly obscure, may in the future be at least partially disclosed.

A spectral change quite as extraordinary, and still more remote from the possibility of anticipation, has just been announced from Potsdam. The fine double star ζ Boötis has long been suspected of capricious and evasive fluctuations in lustre. Herschel, who discovered the pair in 1796, noted the following component as the brighter, and so it appeared to Struve until 1833, when the superiority became inverted. In the 'Mensuræ Micrometricæ'

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[*] *Astr. Nach.* no. 2883.

[†] *Month. Notices*, vol. xlix. p. 303.

[‡] A. W. Clayden, *Month. Notices*, vol. lxvi. p. 58.

(p. 21) he registered them as of 3·5 and 3·9 magnitudes, adding the remark: "Splendor in alterâ stellâ est variabilis." Their instability was verified by Secchi and others; and Dunér, whose observations extended from 1868 to 1875, considered them to vary in concert, not by turns, from 3·3 to 4·4 magnitudes *. That is to say, their equality was consistently maintained, though at different levels. Their light is of the Sirian quality usually given by variable double stars. Their mutual revolutions progress at an inordinately slow rate. During the first forty-five years of astronomical acquaintance with them, they appeared stationary; then at last a wheeling movement became apparent, but of so leisurely a character that, unless vastly accelerated, it will require millenniums for the completion of a circuit. Meanwhile, the stars are gradually drawing together, the interval between them having narrowed to about half its original value of one second. That they are immeasurably remote from the Earth is rendered probable by the minuteness of their proper motion, as well as by their inactivity in revolution. They gave little promise, accordingly, of repaying the labour of sustained watchfulness. Careful measurements at long intervals might have been expected to supply an ample record of their tardy displacements. Nothing in their spectra indicated the likelihood of considerable or abrupt physical change. The unforeseen has nevertheless come to pass. A spectrogram taken June 3, 1905, disclosed to the scrutiny of Drs. Ludendorff and Eberhard a conspicuous development of bright bands †. Still more wonderful to relate, these had faded out completely two days later, when a fresh impression was secured. A spectrum of the kind associated, as a rule, with the outburst of a Nova had thus been, as it were, casually appended to a normal Sirian star, apart from any marked increase in its general light. The mode of its origin is hence a baffling enigma. Nor is there any means of determining whether the blaze was confined to one of the stellar atmospheres, or was common to both. The system must indeed, by any reasonable estimate of its mass, be exceeding spacious; yet its dimensions are so contracted by distance that only a joint spectrum can be photographically derived from it. The bald fact then confronts us that an ordinary double star, variable to a trifling and by no means uncommon extent, put on for a brief space the flagrant aspect of a temporary sidereal object. The change in ζ Boötis was, however, clearly not cataclysmic. It ensued through no portentous accident, but in seeming tranquillity, occasioned by some evanescent influence, external or internal. Can it be, we involuntarily ask ourselves, that genuine Novæ are similarly produced? Is their evoking cause the same in kind, though enormously reinforced in power? The question is for the present unanswerable. All we can do is to keep an open mind on the subject of stellar apparitions.

A. M. Clerke.

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* 'Mésures Micromètriques,' p. 68.

† *Astr. Nach.* No. 4067.

The British Association in South Africa.

[Continued from p. 134.]

"It may seem strange," began Dr. Backlund, "that I should tell you about geodetic and gravitational measures on Spitzbergen, between the parallels of 76° and 81° north latitude, when we are assembled 32° south of the equator"; but the excuse was, of course, that geodesy is just now a question of very special interest in South Africa. As a contribution to the figure of the *ellipsoid* an arc of 5° can be of little interest, compared with that of the arc of meridian of about 108° which will be available when the Cape to Cairo arc is eventually joined to the great Russian arc ending at Hammerfest, at the beautiful monument on the north shore of the bay, which will be remembered by all who went to Vadsö for the eclipse of 1896. But the ellipsoid and the spheroid in geodesy are getting out of date; the real thing nowadays is the *geoid*, and it is as a contribution to our knowledge of the geoid that the work in Spitzbergen was undertaken jointly by the Russian and Swedish Governments. Eighty years ago Sabine had recognized its importance, and had swung pendulums in the north of Spitzbergen. But geodetic measures were impossible until Jäderin's invention of his wires for base measurement avoided the principal impossibility—the measurement of a base by the old methods when the only level ground was knee deep in mud and water.

A first summer was spent in reconnaissance; the following winter in magnetic and pendulum work; but the summer which should have seen the triangulation completed was "very severe"; the whole coast was icebound, and only two points could be occupied. The parties became disheartened, and the enterprise looked like being a failure. But early next summer Dr. Backlund borrowed the famous ice-breaker from Admiral Makaroff, and arrived in great style in June to take command in person. With these powerful reinforcements all went well. A base of 6¼ kilometres was measured twice with the Jäderin wires in 48 hours; the whole arc of 272 km. was measured with a probable error $\pm 4^{m}\cdot 6$; and when the representatives of the two parties met at Pulkowa to compare the lengths they had separately deduced for the side common to their triangulations, the discordance between them amounted to 6 cm. on a length of 80 km. The islands are divided by a deep fjord. On one side are mountains, and there the geodetic azimuths agree with the astronomical, but the latitudes are discordant by 5″, of which only half can be accounted for by the attraction of visible masses; on the other side of the fjord the islands are flat, and there the latitudes are concordant, but the azimuths discordant.

The strong point of this work, said Sir David Gill, is in the fact that astronomical latitudes were observed at every station, and the value of such a precaution may be judged by the case of

Port Elizabeth, where the geodetic and astronomical latitudes differ by 19″, which is not to be accounted for by any visible masses. There is no possibility of joining the African to the Spitzbergen arc by triangulation, but there is, nevertheless, a link between them in the person of Dr. Rubin, who was head of the Swedish party near the North Pole, and is now in charge of the field operations which are nearing the equator from the south.

With the retirement of Mr. Tebbutt from active work, and the departure of Mr. Innes from the Cape, to become a meteorologist, the observation of double stars in the southern hemisphere came to a standstill precisely at the time when it was particularly desirable to extend to the South Pole the survey of all stars down to the 9th magnitude which has been made by Messrs. Hussey and Aitken in the north. Innes has a fine eye for close doubles; he must be recalled to astronomy, and the Government of the Transvaal must be persuaded to allow his friends to enlarge the scope of the Government Observatory at Johannesburg by providing a big refractor for that splendid climate. So the way was made smooth at Cape Town for a function twelve days later at Johannesburg, which will perhaps be counted in the future as the beginning of a new era in southern astronomy. Mr. Frank Dumat, a Fellow of the R.A.S., entertained all the astronomers to lunch at the New Club, whence a procession of motors took the party out to the stony eminence on which Mr. Innes has installed all the latest of self-recording devices, even to a lightning recorder whose Marconi receiver begins to write whenever a thunderstorm within a hundred miles sends a wireless message to say it has begun. Presently his Excellency the High Commissioner arrived to see the observatory, and after a while the party strolled to the top of a knoll, which did not seem to be of any particular use for meteorology. Suddenly a brilliant idea occurred to everybody simultaneously. What a place this would be for a telescope! The idea seemed so good that some perhaps deliberately, and others certainly unexpectedly, found themselves making speeches; to which his Excellency was sympathetic, said that the Government could not be expected to find money for astronomical research when they had not half enough for elementary schools, but thought that they would no longer object to the gift of a telescope for this important project.

The High Commissioner rode away, and the party returned to the city to hear the conclusion of the President's address, for which Lord Selborne moved the vote of thanks, and, to the great pleasure of all, told how he had spent his afternoon, and how much he had been impressed by the possibilities for scientific good which could be reached if citizens who had found wealth in Johannesburg would use some of it in the way that had been suggested.

In Johannesburg, as in Cape Town, the interest of Section A was very largely astronomical and geodetic. Sir David Gill described the operations on the great African arc of meridian in a paper which, itself a summary, cannot be summarized here without leaving out most of the points. A few are so striking that they must be retained. The Hoots River base has been measured with an apparent accuracy of one in seven million, while it is impossible to rely on the absolute standard of length to within one part in a million. The latest results for the P.E. of an angle are about $\pm 0''\cdot 25$. In a length of 500 miles a chain develops an error of about 1 in 60,000; but with the new Jäderin apparatus bases can be measured with ease every 200 miles, and the new nickel-steel wires retain their length almost unaltered by secular change, so much has the process of annealing been improved. The Chartered Company have loyally carried out, in times of exceptional difficulty, their promise to provide for the measurement of the arc up to the boundary on Lake Tanganyika, whence the Germans will take it on. But there is a small gap between the Rhodesian and the Transvaal sections which has yet to be covered, and the Chartered Company can do no more. The trained parties in the South are being dispersed and their skilled services lost because the Government cannot find money for the topographical survey. Some economy, the President of the Section remarked, is a mere loss of money, and it will be a very false economy to stop short at the point where the Survey begins to be of economic value.

Another paper which almost defies summarization was that of Prof. E. W. Brown, describing the present condition of his Lunar Theory and the prospects of forming tables from it. The theory is complete so far as the solar perturbations are concerned: the planetary part will, it is hoped, be finished in two or three years; but prophecy is dangerous, because one cannot tell *à priori* whether the terms will be large or small. The natural outcome of the theory is to form tables, which are much more expensive than the theory. Provisional tables must be formed with the adopted constants, and these must be compared with observation to get corrected values of the constants for the final tables. *The results of the comparison of one theory with observation will not furnish the constants for another theory.* The constant found by comparison with Hansen could not possibly be accepted as final, but could be of use only in forming the provisional tables. A weighty paper, for obvious reasons full of interest at the present moment, but lightened by one of the most curious tricks of memory imaginable. The subject of the construction of tables of the Moon led naturally to reminiscences of previous enterprises of the kind. We were told that the British Government offered a large sum of money, and the astronomer died just before his work was completed, and how it ever got completed was not quite clear, and the widow only received a portion of the money; and in the end it turned out that the speaker had been saying Hansen and meaning Mayer all the time!

With the final meeting at "Jo'burg" on the last day of August the formal business of the Association came to an end, and nothing more serious remained than evening lectures for those who could not escape by going trekking. For a while the Association, and especially the Astronomical Society of the Atlantic, got much scattered. Part of it went determining the brightness of the Milky Way at night upon the veldt, with a cigar and a visiting-card; one member went ahead to Bloemfontein to meet people who expressed themselves passionately desirous of learning about the Straits of Magellan; another had been left behind all the while in Cape Town finding out something about the heliometer. He never rejoined, but the others came back one by one till the party was almost complete at Mafeking; and perhaps the Editors will allow them next month a final appearance as a Society in these pages.

A. R. H.

An Inscription on an old Dutch Snuff-box.*

4	11	.	12	4		23	2	
5	9	.	13	3	...	26	1	.
6	7	...	14	3	..	31	1	.
7	6	...	15	3	.	37	1	.
8	6		16	3		45	1	
9	5	.	17	2	...	65	0	.
10	4	...	19	2	..	100	0	.
11	4	.	21	2	.	200	0	.

The inscription consists of three columns, the first two columns being composed of numbers and the third column of dots or blanks. The numbers in the first two columns may be supposed to represent multiples of some unit, this unit being the same for the same column but not necessarily the same for different columns. The number of dots in each horizontal row may also be supposed to represent a multiple of some unit, this unit being the same for all the rows of dots, but not necessarily the same as either of the units for the first two columns. On examining the third column it is seen to consist of an irregular series of dots together with occasional blanks, the number of dots never exceeding three.

* [The explanation of the appearance of a non-astronomical article in this Magazine will be found in the following circumstances:—There was submitted to a member of the Greenwich staff an antique box, said to have been used for snuff, evidently of Dutch origin, on the bottom of which the three columns of figures and dots as shown were cut, with some idea that the inscription was of astronomical significance, and that he might be able to interpret it. This our colleague was unable to do, but he submitted it in turn to Major MacMahon, who has been appointed to the position of Deputy Superintendent of the Standards Department, Board of Trade, an office formerly held by the late Mr. Chaney. Mr. H. B. C. Darling, one of Major MacMahon's staff, was not long in solving this unpromising problem in the ingenious way here detailed.—Eds.]

This leads to the idea that the unit for the second column may be four times that for the third column. Some support is afforded to this view by the fact that if the dots represent quarters and the numbers in the second column represent whole units, we have, proceeding upwards from the 200, an unbroken succession of consecutive numbers of quarters extending over the greater part of the combination of these two columns, thus :—

Number in first column.	Combination of second and third columns, expressed in quarters.
200	1
100	2
65	3
⋮	⋮
11	17

At 10 this sequence is broken.

None of the numbers in the second column exceeds eleven, but as these numbers form a regularly increasing series (reading upwards), we cannot draw from this fact any conclusion as to the relation between the units for the first and second columns.

Regarding the unit for the third column as one-fourth that for the second, let us now express the third column in terms of the unit for the second column. Thus for one dot we should put $\frac{1}{4}$, for two dots $\frac{2}{4}$ or $\frac{1}{2}$, and for three dots $\frac{3}{4}$. The first few rows would then read : —

4	$11\frac{1}{4}$
5	$9\frac{1}{4}$
6	$7\frac{3}{4}$
7	$6\frac{3}{4}$

Now the product of 4 by $11\frac{1}{4}$ is 45, the product of 5 by $9\frac{1}{4}$ is $46\frac{1}{4}$, the product of 6 by $7\frac{3}{4}$ is $46\frac{1}{2}$, and it will be found that the products corresponding to all the several different rows vary irregularly between 45 and 50, and thus, considering their number, may be regarded as fairly constant.

This suggests the idea that the inscription may be intended as a table for the purpose of indicating the weights corresponding to the various divisions on the graduated arm of a weighing instrument. Let us first consider a steelyard having a fixed counterpoise and a movable carrier to contain the load. We suppose that the numbers in the first column represent numbers engraved against the graduated divisions on the arm of the steelyard, and that the numbers in the second column represent respectively the values corresponding to these divisions expressed in terms of some unit of weight, the third column representing the quarters of this unit. For the sake of brevity of expression, let us endeavour to choose this unit from some recognized system. The only condition limiting our choice is that this unit must be four times another recognized unit. This condition is, however, sufficiently restrictive if we consider systems of weight only, and limits us to selecting either hundredweights and quarters in the avoirdupois system, or carats and carat-grains in the system adopted by dealers in precious stones.

The latter system is here indicated as the more probable, for in the case of cwts. and qrs. the weighing instruments would necessarily have to be somewhat massive, and there would be plenty of space to mark the values corresponding to the various divisions against the respective numbers on the arm. We will therefore adopt the jewellers' system. Now, although the weight represented by the name "carat" is not the same in all countries (*e.g.*, in Great Britain 3·16831 avoir. grains go to the carat, whilst in Holland the carat = about 3·0 of these grains), yet the practice of subdividing the carat into four quarters is practically universal. In Great Britain these quarters are called "carat-grains" (1 carat-grain = 0·792 .... avoir. grain, 1 avoir. grain = $\frac{1}{7000}$ of the Imperial Standard Pound).

Returning to our steelyard, let us consider at what distance from the fulcrum the divisions on the graduated arm are to begin, and also what is to be the moment about the fulcrum of the weight of the steelyard and empty carrier. The following values for these constants are readily indicated, viz.:—Distance of the zero division from the fulcrum = 0·3 units of length on the side of the fulcrum opposite to that on which the centre of gravity of the steelyard and empty carrier is situated, and moment about the fulcrum of the weight of the steelyard and empty carrier = 195·6 (carat-grains × units of length). This moment is constant if the steelyard is always horizontal in the position of equilibrium, as is generally the case, this position being indicated by a vertical pointer rigidly attached to the steelyard above the fulcrum, and oscillating across a fixed mark on the loop or shackle supporting the instrument at the fulcrum.

Let us suppose that the divisions on the arm are equidistant and are represented by the numbers 0, 1, 2, 3, 4, 5, &c. up to 200. Thus there are 201 dividing lines in all. The distance between any two consecutive graduations is assumed to be one unit of length. The first division mentioned in the inscription is that numbered 4. This is situated therefore at 4·3 units of length from the fulcrum; and for equilibrium the weight which must be placed in the carrier when it is set at this division is consequently

$$\frac{195{\cdot}6\ (\text{carat-grains} \times \text{units of length})}{4{\cdot}3\ \text{units of length}} = 45\tfrac{21}{43}\ \text{carat-grains.}$$

Now the inscription does not recognize any unit lower than the quarter or carat-grain, so that, taking the nearest whole number of carat-grains, we have for this division the value 45 carat-grains, or 11 carats 1 carat-grain, which is represented by 11. in the inscription.

Proceeding in this way, that is, dividing the number 195·6 successively by the numbers in the first column of the table increased by 0·3, and taking the quotient to the nearest integer, this integer gives the number of carat-grains equivalent to the carats and carat-grains indicated by the figures and dots in columns 2 and 3, and the suggested explanation that the inscription was a table of weights to be used with a small steelyard in trading diamonds seems to be not an improbable one*.

H. B. C. DARLING.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Nautical Almanacs and Ephemerides.

GENTLEMEN,—

The origin of the word "Almanac" has been much disputed. The first syllable suggests an Arabic source, and this is, perhaps, the most probable, though Littré allows that something is to be said for an Egyptian (Coptic) derivation. An almanac, however, generally includes information other than astronomical, and the word "Ephemeris" is usually adopted as best expressing the daily and seasonal guides to the places of the heavenly bodies. From the first the *Nautical Almanac* bore as additional title "and Astronomical Ephemeris." (I may remark parenthetically that it never had the unnecessary final *k* in the word Almanac, which died rather hard in English, whilst in France and Germany there has always been a final *ch*, as coming mediately from the Greek χ.) The origin of the word Ephemeris is quite obvious, but it may be of interest to some to call attention to the circumstance that the first known use of it is in Plutarch, who applies it to what are commonly called the "Commentaries" of Julius Cæsar.

The first, however, of the great national ephemerides had a different title altogether. It was founded by Picard in 1679, and called 'La Connaissance des Temps ou Calendrier et Ephémérides,' but this was afterwards changed into 'Connaissance des Temps ou des Mouvements célestes.' Picard started it in 1679 (editing the first five volumes), and it has appeared regularly ever since, the volume for this year being the 228th.

It was not until nearly a century later that the first *Nautical Almanac* appeared. Maskelyne started it in 1767, the first volume being for the year 1768, and he edited it till his death. It is unnecessary here to refer to the other works of Picard and Maskelyne, though one cannot help just mentioning the well-known facts that the former measured the Earth and the latter weighed it.

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* [Mr. Darling goes on to discuss the case of the weight of the carrier or scale-pan for the diamonds not being neglected, but we regret that space will not permit us to print this.—EDS.]

The third great national ephemeris emanated from Berlin, and was practically founded by Bode, whose name appears in the first volume as the calculator of the ephemerides. The date of that volume is 1774, and it is for the year 1776. The original title is 'Astronomisches Jahrbuch oder Ephemeriden für das Jahr 1776,' which Encke changed in 1830 into simply 'Berliner Astronomisches Jahrbuch.' Bode had edited it for more than fifty years. His star-charts (uranography) and star-atlas are familiar to all astronomers. But the general public best know his name in connection with the so-called law of planetary distances, usually called Bode's law, but often of late years attributed to Titius, of Wittenberg, though he had really been anticipated in this by Christian Wolf, who died at Halle in 1754. Of course, that there was a gap between the orbits of Mars and Jupiter had been noticed by Kepler long before that, and the discovery of Neptune has shown that the progression is not a law through the system. In speaking of Bode, one cannot omit to mention his 'Anleitung zur Kenntniss des gestirnten Himmels,' an excellent elementary treatise of which an eleventh edition was published by Bremiker in 1858. But much of it is now obsolete, referring to planetary positions and phenomena between 1858 and 1868. Bode was the first to recognize in the moving star discovered by Piazzi a new planet, afterwards named Ceres. He also suggested the name Uranus for Herschel's planet.

It is only desirable to mention here two other national ephemerides, which date their commencement from the last century. The 'Almanaque Nautico y Efemérides Astronomicas,' adapted to the Naval Observatory of San Fernando, near Cadiz, appeared first in 1827 (edited by Don Cerquero*), for the year 1830. 'The American Ephemeris and Nautical Almanac,' which now occupies so high a place amongst these annuals, was first published at Washington in 1852 for the year 1855. The editor (the late Admiral C. H. Davis) has stated that the work for it was begun in the latter part of 1849, and was much facilitated by the labours and library of the late Dr. Bowditch, best known for his translation with comments of the 'Mécanique Céleste' of Laplace.

Yours faithfully,

Blackheath, 1906, Mar. 1. W. T. Lynn.

Ancient Eclipses and the Lunar Theory.

Gentlemen,—

I have been much interested in Mr. Cowell's researches on the motion of the Moon, and more particularly in the use which he has made of the eclipses recorded in the 'Almagest.' Ptolemy's records seem to be entitled to much greater weight than they have recently been given by astronomers. For some reason not easy to explain, these eclipses have been almost ignored in recent studies of the lunar motions, whereas they seem to be the very best records antiquity has bequeathed to us. It may not be out of place to mention the fact that before any of Mr. Cowell's work appeared I had studied the eclipse records in the 'Almagest' carefully, with a view of ascertaining the probable uncertainty of the Moon's place at that epoch, as inferred from these records. Early last year I reached the conclusion that they have been decidedly underrated by recent writers, and that they should give the secular acceleration with an uncertainty not much greater than 1″.

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* He died in 1850, having been Director of the Observatory 25 years.

After making this examination into the circumstances of the eclipses of the 'Almagest,' I was surprised to find that Prof. Newcomb had apparently assigned greater weight to the few observations of Ibn Jounis, which are so much nearer our time, than to the numerous remote records of the Greeks. It seemed impossible to justify such procedure on any valid ground. The ground often taken of rejecting or giving small weight to certain observations because they do not fit in with a theory is always a suspicious proceeding, to be adopted even temporarily only as a last resort. In the present case, Prof. Newcomb's course recalled to mind that adopted by an illustrious mathematician of the 18th century. When Lagrange failed to find the cause of the Moon's secular acceleration he too discredited the fact discovered by Halley by rejecting the ancient observations, while Euler ascribed the supposed effect to the resistance of the ethereal fluid. Laplace had the sagacity to adopt a different course, and the discovery which followed is one of the great historical achievements of physical astronomy.

In the same way Prof. Newcomb rejects the former well-authenticated redness of Sirius, because he cannot explain the fact. But the ink was scarcely dry on his new book on the stars, in which he takes this position, when Nova Persei blazed forth in 1901; and observers saw it change colour from day to day and week to week. Could any one explain the cause of these numerous and conspicuous changes of colour? Shall we, then, deny the changes of colour in Nova Persei, some of which were noticed when it was nearly as bright as Sirius?

The proper course seems to be to adhere to the records in all cases, unless we have absolutely conclusive proof that they are wrong. In this way we make discoveries which otherwise would escape us for ever. Lalande's rejection of an observation in 1795 cost him the discovery of Neptune.

Yours faithfully,

T. J. J. SEE.

Naval Observatory, Mare Island, Cal.
1906, Mar. 9.

Bradley's Lunar Observations.

Gentlemen,—

I hope I shall not be thought to be making a mountain out of a mole-hill by writing a letter to call attention to an erratum. But the matter is of some importance historically. It is commonly thought that Bradley's lunar observations were not reduced until Airy undertook the great work of reducing the planetary and lunar observations of his predecessors from the year 1750, for which he received the Gold Medal of the Royal Astronomical Society in 1846 and one of their Testimonials in 1848. But, in the early years of his rule at Greenwich, Maskelyne (who always showed special diligence in all matters connected with lunar observations) published a series of the results of Bradley's lunar observations, to which I alluded in the P.S. of my letter to you of February 6 (vol. xxix. p. 140), where an unfortunate misprint represents them as commencing in 1760 instead of 1750. The reduction, published by Maskelyne as an Appendix to the *Nautical Almanac* for 1774, contains 1220 longitudes and latitudes of the Moon from Bradley's observations with Bird's new meridian instruments between September 13, 1750, and November 2, 1760. They are also compared with a set of manuscript tables improved by Bradley from Mayer's. The greater part of the reductions were performed by Bradley himself, assisted by Charles Mason, what was left unfinished being subsequently completed by Mason at the instance and expense of the Board of Longitude. These are also printed (without the comparisons or statement of source) as Table X. in the Introduction to the first volume of Bradley's observations, which was published by Hornsby in 1798. That volume included the observations until 1755. The second (which contained also those of Bliss from 1762 to 1764) did not appear until 1805, edited by Abram Robertson, then Savilian Professor of Geometry at Oxford.

Yours faithfully,

Blackheath, 1906, Mar. 6. W. T. Lynn.

OBSERVATORIES.

Melbourne.—From the Report of the Board of Visitors to this Observatory for the year ending 1905 March 31, it appears that no steps have yet been taken by the Government for filling the vacant post of Chief Assistant, although the Board think it a matter of vital importance that this post should be filled by a young and well qualified astronomer.

The astronomical work still continues to be mainly astrographic. With the transit-circle 2258 observations of R.A., 1442 of N.P.D. were made during the year, chiefly of stars chosen to serve as fundamental for the reduction of the plates. The preparation of the Fourth Melbourne General Catalogue is proceeding, but this cannot be completed until all the requisite observations of these fundamental stars have been made.

The state of the photography at date of the Report, 1905 March 31, was as follows:—One series of Catalogue plates, 1149 in number, was complete; of the second series 455 were taken. Of the Chart plates 565 with centres at even degrees of declination, and 495 with centres at odd degrees of declination, were taken, which leaves 89 to complete the Melbourne zone. The Catalogue plates are being measured in cooperation with the Sydney Observatory, and at the above date the numbers measured were: Sydney 317 plates, containing 177,069 stars; Melbourne 612 plates, containing 206,604 stars.

An important work now going on is the reduction and preparation for publication of past records of magnetic elements extending back for 37 years. The total number of such photographic curves measured is 37,212, the number still remaining being under 3000. This measurement will be completed shortly, and the preparation of the whole work for publication will be begun.

Other miscellaneous work of time service, meteorology, seismology, standardizing of weights and measures, and tide-recording have been continued. Four hundred and six visitors were received during the year, mostly on Wednesday afternoons.

Incidentally, it may be learnt from this Report that the annual cost of the Department is £4365, and that the capital value of the land, buildings, and equipment is estimated at £70,300. The question whether the control of the Australian Observatories shall be taken over by the Commonwealth Government is still undecided.

NOTES.

Comet Notes.—The last few months have been an active time in the cometary world. Last month two further discoveries were made—1906 *b* by Herr Kopff at Heidelberg on March 3, 1906 *c* by Mr. Ross, of Melbourne, on March 18.

The first was of magnitude 10½, and was discovered photographically in the course of their work on minor planets. Photographs obtained at Greenwich show a very well-defined outline and little nebulosity.

The second was of the 8th magnitude, about 3′ in diameter, with some central condensation; it was rather badly placed for European observers, but is coming North. Unfortunately both these comets have passed their perihelion, and are now receding from both Sun and Earth, and steadily fading.

The following are the orbits (very similar elements of 1906 *c* have been computed by Miss Lamson):—

	Comet 1906 *b*.	Comet 1906 *c*.
Calculator	M. Ebell.	E. Strömgren.
Observations	March 3, 9, 17.	March 19, 20, 21.
T (Berlin M.T.) ..	1905 Nov. 5·434	1906 Feb. 22·216
ω 1906·0	160° 35′·7	281° 24′·1
☊ 1906·0	340 59·4	73 2·1
i 1906·0	3 41·6	80 34·0
log *q*	0·4913	9·8834

The perihelion distance of 1906 *b* (about 3·1) seems to be the second greatest on record. It was investigated whether the orbit might be elliptical, but a period of 1153 years was found, showing that there is little, if any, deviation from a parabola.

Ephemerides for Berlin Midnight.

	1906 *b*.			1906 *c*.		
	R.A.	N. Dec.	Brightness.	R.A.	N. Dec.	Brightness.
	h m s	° ′		h m s	° ′	
April 2...	11 23 59	2 21	0·83	2 48 40	7 38	0·53
6...	11 22 53	2 24	0·81	2 58 9	10 47	0·44
10...	11 21 59	2 27	0·78	3 7 9	13 41	0·36
14...	11 21 18	2 28	0·74	3 15 47	16 21	0·29
18...	11 20 51	2 28	0.70	3 24 8	18 49	
22...	11 20 38	2 26	0·67	3 32 15	21 10	
26...	11 20 40	2 24	0·64			
30...	11 20 58	2 20	0·61			
May 4...	11 21 30	2 14	0·57			

A few observations of Comet 1905 *c* Giacobini have been obtained since it emerged from the Sun's rays, but it was not a conspicuous object. Herr Wirtz, of Strassburg, observed it on March 17, and described it as of magnitude 11½, an approximately circular nebulosity about 2′ in diameter, without nucleus. It may still be visible with large instruments in April, so Herr Wedemeyer's ephemeris is continued (for Berlin midnight):—

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Apr. 3....	3 32 18	7 41	Apr. 27....	4 19 38	12 25
9....	3 45 22	9 8	May 3....	4 29 49	13 15
15....	3 57 30	10 23	9....	4 39 32	13 59
21....	4 8 53	11 28	15....	4 48 47	14 37

Brightness on May 15 $\frac{1}{20}$ of that at discovery.

Comet 1906 *a* Brooks is likewise very faint, but possibly still within reach of large instruments. The following revised elements are by Herr M. Ebell from observations on Jan. 27, Feb. 3, 10:—

T	1905 Dec. 22·224 Berlin M.T.	
ω	89° 43′ 47″	1906·0.
☊	286 22 2	1906·0.
i	126 27 33	1906·0.
log *q*	0·11242	

Ephemeris for Berlin Midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Apr. 3....	5 47 2	38 21	Apr. 21....	5 58 32	31 48
9....	5 50 34	35 48	27....	6 2 49	30 11
15....	5 54 26	33 39	May 3....	6 7 13	28 46

Brightness on May 3 $\frac{1}{20}$ of that at discovery.

The Danish Royal Academy of Sciences offers a prize for the best essay on the theory of Faye's Comet from 1873–1896. Essays to be sent to Copenhagen before October 1908 (*Ast. Nach.* 4080).

A. C. D. C.

MINOR PLANET NOTES.—A large number of discoveries have been made; those by Mr. Metcalf were made at Taunton, Mass., U.S.A., SD^a at Paris, the remainder at Heidelberg.

Planet.	Date.	Discoverer.	Planet.	Date.	Discoverer.
SZ ...	1906 Feb. 16	Wolf.	TJ ...	1906 Feb. 16	Metcalf.
TA ...	16	Kopff.	TK ...	16	”
TB ...	16	Wolf.	TL ...	22	”
TC ...	21	”	TM ...	Mar. 3	Kopff.
TD ...	21	”	TN ...	3	Wolf.
TE ...	16	Metcalf.	TO ...	4	”
TF ...	22	Wolf.	HV^a...	1902 Mar. 5	”
TG ...	22	”	SD^a...	1905 Nov. 3	Lœwy.
TH ...	22	”			

400, SF have been named Ducrosa, Tauntonia respectively.

TF, TG were remarkable for their rates of retrogradation, 90 sec. and 30 sec. daily; the former is unusually large and probably indicates a small mean distance, the latter is unusually small. SD^a was found on one of the Astrographic Chart plates taken at the Paris Observatory.

A. C. D. C.

A CRITICISM OF SIR GEORGE DARWIN'S THEORIES.—In the *Astronomical Journal*, No. 581, Mr. J. N. Stockwell contributes a criticism "on the Darwinian Theory of the Genesis of the Moon and on Tidal Evolution." An attempt is made to overthrow the theory altogether, but we find that not one of the arguments is admissible, and, especially in the latter part of the paper, blunders are numerous.

Dealing first with the birth of the Moon, the author arrives at the result that if a particle were thrown off the Earth by centrifugal force, the Sun's action would cause a perturbation of four feet in the radius vector. One is tempted to smile at the seriousnes with which Mr. Stockwell deduces the overthrow of a great theory from a perturbation of four feet. He states that this would cause the particle to be precipitated to the Earth again: that is not necessarily true; the radius vector may be a minimum instead of a maximum at the instant of projection. But in any case Prof. Darwin never supposed that the matter forming the Moon was thrown off by centrifugal force alone. He suggested that the solar tides might be the cause, when they were exceedingly great, through synchronizing with the period of free vibration of the Earth.

Here, and in fact throughout the paper, the author fails to realize the complexity of the problem he is attacking. In several places he attempts, by application of particle dynamics to roughly analogous problems, to overthrow results which have been obtained by the most careful and rigorous analyses of the actual problem.

Mr. Stockwell's calculations are not always accurate; he mentions a period of 3,456,000,000 years, which should really be 6000 years. He had forgotten to take a square root.

Nor is he more fortunate in his abstract reasoning. He appeals to the principle of action and reaction to show that a negative secular equation of the Moon's motion must produce an increase in the Earth's velocity of rotation. It is hard to conjecture how he arrived at a result so obviously contradictory to the law of conservation of angular momentum, and, in fact, to the principle of action and reaction. Is it possible that he was considering his tidal spheres (with which he replaces the tides) to be rigidly attached to the Earth's surface instead of following the apparent motion of the Moon?

After this we are hardly surprised to find the author solemnly proving that the tidal tangential forces do not exist at all. This amounts to saying that the attraction due to a spheroid is everywhere radial!

In another place Mr. Stockwell completely misrepresents Prof. Darwin. He says, "but he roughly estimates the time at which the Moon was in close proximity to the Earth as sixty million years." What Darwin does say is: "It may be proved in fact that if tidal friction always operated under conditions most favourable for producing rapid change, the sequence of events from the beginning until to-day would have occupied a period of between 50 and 60 million years. *The actual period of course must have been much greater.*"

The question of the time required by the Darwinian theory is one of very great importance, and perhaps it is not as well realized as it should be how much more probable the theory has been rendered by the recent revolution in our estimate of the age of the Earth. When Lord Kelvin's estimate of the Earth's age was accepted, it seemed doubtful if the Earth-Moon system could have been evolved by tidal forces in the limited time that he allowed. Now the discovery of radio-activity has changed our ideas altogether; the assumptions on which Lord Kelvin's calculation rested are known to be untrue. We can assign no upper limit to the age of the Earth, and the thousands of millions of years demanded by geologists are freely conceded. Having so much longer time to act in, the importance of tidal forces is increased; and should they amount to a tenth or even a hundredth of Ferrel's estimate, we should be almost forced to the conclusion that the Earth and Moon were in close proximity at a time when the former was still a compact and dense planet. Darwin's theory would thus be not merely admissible but inevitable. A. S. E.

STAR-STREAMING.—Prof. J. C. Kapteyn has kindly sent us a copy of his memoir with this title, excerpted from the Report of the British Association meeting of last year, of which an admirable summary was given on page 132 of last number. To this may be added Prof. Kapteyn's remark that the crucial test of his whole theory lies in the observations of radial velocities, such as he expects are already made by American astronomers, but are lying waiting publication. He has delayed publishing his theory, hoping to have such determinations of radial velocity at his command, but now brings it forward in the expectation that it may elicit what he wishes.

Meanwhile, Prof. Kapteyn has used such material as he has at his disposal, which consists of the spectroscopic observations of the velocity of 78 stars—the Orion stars are excluded because they appear to be nearly at rest in space—and the observations of 46 spectroscopic binaries. These show the phenomenon of the star-streams less strongly than the fainter stars, as was expected but still they give confirmation of Prof. Kapteyn's hypothesis.

A THEORY OF THE MILKY WAY AND THE CLOUDS OF MAGELLAN.—A short paper by Mr. A. R. Hinks in the 'Proceedings of the Cambridge Philosophical Society' for November 13 last, which may be taken as supplementary to a lecture on the same subject delivered by the author in South Africa last year, has for its thesis the facts that in the northern hemisphere the stars in general, the coarse clusters, the gaseous nebulæ, and the stars with bright line spectra of type V. are concentrated upon the plane of the Milky Way, while the spiral nebulæ seem to avoid it. In the southern hemisphere the same classes of bodies are concentrated in the Milky Way and in the Greater Cloud of Magellan, but the spiral nebulæ, which appear to avoid the Milky Way as in the north, crowd into the Greater Cloud of Magellan to meet those Milky Way constituents with which they are asssociated nowhere else.

These facts have by others been taken to support the proposition that stars are symmetrically condensed upon the plane of the Milky Way, the spiral nebulæ symmetrically condensed about its poles, and that hence it is necessary to look for some separative influence which had driven these stars and spiral nebulæ apart, except in one particular region of the sky.

The author of this paper is not inclined to adopt this view, but suggests as an hypothesis that the stars and other bodies forming the Universe are distributed in independent clouds roughly in one plane. The cloud in which our Sun is situated would then supply the stars of sensible proper motion, which are uniformly distributed; the neighbouring clouds would supply the bulk of the stars down to the eleventh magnitude, which are, to a considerable extent, distributed about centres in the galactic plane; the more distant star-clouds would make the Milky Way proper as seen by the unaided eye.

The Smaller Cloud of Magellan would, on this hypothesis, be one of the distant star-clouds which happened to lie far out of the general plane. The Greater Cloud of Magellan may perhaps be more properly considered a nebula and star-cluster cloud than a star-cloud.

It is suggested in the note that the distribution of known nebulæ may be expressed thus: they are in clouds which do not lie in the plane of the star-clouds, but, on the other hand, are not aggregated towards the pole of that plane. The Greater Cloud of Magellan is a cloud of particularly rich constitution, including numbers of Milky Way objects, as well as numbers of nebulæ which are probably spiral (although there is no proof of this), but there need be no reason to suppose that it has any special structural significance.

If Mr. Hinks's view be taken as a working hypothesis, it follows that in statistical investigations of the distribution of stars and nebulæ, grouping must be looked for instead of any possible arrangement depending on galactic latitude and longitude, or any distribution in great circles.

SHADOW-BANDS.—The number of the Reports of Eclipse Expeditions of August last precludes complete notice of them in the limited space at our command; but the report of Dr. Raoul Gautier, of the Geneva Observatory, claims attention because of an account of some observations of the phenomenon of shadow-bands which are a little unusual. Dr. Gautier observed the eclipse from Santa Ponza, in the Island of Majorca, and was not favoured with particularly good weather; in fact, like other observers in the island, he saw only the inner ring of the corona. Among his party was M. Forel, well known as a skilful observer of natural phenomena, who undertook the observations of a physical nature, and among them that of shadow-bands. This is M. Forel's report:—

> The weather was not favourable: the sky was covered with clouds of varying thickness, from a simple whitish veil, up to an opaque screen entirely masking the Sun. A rather strong wind blew from the south-west.
>
> Some seconds before totality, and for some seconds only, we saw shadow-bands in the form of wavy lines not at all sharp; the most accentuated were distant one from the other by about perhaps a metre, and between them lines less well marked, from 3 to 5 centimetres broad, vertical upon the vertical walls, parallel to the meridian on the ground. These bands were affected by a motion of trepidation, of vibration about their position, but there was no lateral dis-

placement, no running in any definite direction. This last part of the observation—the absence of lateral displacement in a definite direction—we both affirm, M. Fontsere and I, according to what we saw at Santa Ponza. We know this is in contradiction to observations made at Palma, but we adhere to our statement.

It seems advisable to put an authoritative statement like this on record. Dr. Gautier's report is accompanied by a little pamphlet, written by himself and M. Henri Dufour, on the same subject, which recalls an observation made by M. Charles Dufour as long ago as 1852. M. C. Dufour wrote: "At the instant when the first ray of the Sun appeared (at sunrise), immediately I saw for one or two seconds something resembling waves, alternately dark and light, moving on the wall of my room." The authors of this pamphlet, during the last few months, have been looking for, and have seen, this phenomenon, which seems to be akin to the shadow-bands seen in eclipses. These bands are seen when the Sun is rising above the horizon, or passes from behind a mountain or from behind a roof. The bands and their movement are sharply defined up to the instant when about a third of the solar diameter is above the horizon, when the lines are less well-defined. The movement of the shadows varies with the state of the atmosphere, being slow in calm weather and very rapid in a stormy wind, each gust of which seems to cause a hurried movement of the shadows on the wall. The conclusion seems to be that shadow-bands are an atmospheric phenomenon.

MAY we be permitted to call attention to a small alteration in the outward appearance of this Magazine. For many years past the last page of the cover has been occupied by the announcement of one firm almost unaltered throughout, so that the back of the numbers is as well known as the front. Messrs. E. Dent & Co. have now found it necessary to make an alteration, and it seems to us that the new matter in their advertisement is not unimportant. The fact that a firm such as this, whose reputation for good work is unrivalled, and is too valuable to lose, should have been able to meet the demand for a good article at about half former prices, is some answer to the frequent verbal attacks on English commercial enterprise.

WE have again been favoured with copies of the new editions of Mr. W. T. Lynn's handy little books. The thirteenth issue of 'Remarkable Comets,' and the eighth of 'Remarkable Eclipses,' both brought quite up to date, as will be realized from the fact that the latter has a note on the total solar eclipse of 1905, and another on Mr. Cowell's work. These books are too well known to need description, but it may be added that they are each sold for sixpence, and are published by Messrs. Sampson, Low, Marston, & Co.

It seems that Mercury as a naked-eye object has been especially conspicuous in the month just past. Some very fine weather happened at the beginning of the month, when the planet was coming to greatest eastern elongation, and he was seen on several occasions.

The next Meeting of the Royal Astronomical Society will be on Wednesday, April 11 (Good Friday being the 13th); of the British Astronomical Association on April 25.

From an Oxford Note-Book.

In *Ast. Nach.* 4041 Prof. Schaeberle discusses an interesting question (as he has done before). He points out that a particle of gravitating matter ejected from the Sun may take either 64 days or 27 days to reach the Earth according as the velocity of ejection is 381·1 or 382·0 miles per second. In the former case it only just reaches the Earth and falls back; in the latter it proceeds to infinity. For intermediate velocities the paths are, of course, intermediate between these. The striking fact is the very slight change in velocity required to produce so great a difference in the path. (Prof. Schaeberle has not done full justice to the point he has himself called attention to, for in place of the former value (381·1) above quoted, he gives, by some numerical slip, 376·8. For the correction I am indebted to Mr. H. C. Plummer, who has gone through the calculations independently.) The result is that, if we imagine, with Mr. Maunder, a stream of particles issuing from a definite point on the Sun and reaching the Earth (in some way causing a magnetic storm) at time T after ejection, we cannot have T constant without supposing an extraordinarily constant velocity of ejection. But why has Prof. Schaeberle stopped at the velocity 382·0 with which the particle would just reach infinity? There seems no reason why the possibilities should be cut off there. Mr. Plummer has extended the table as follows (his assumed data doubtless differed a little from Prof. Schaeberle's, and hence the results differ a little):—

Velocity of ejection.	Time to Earth's orbit.	Velocity of ejection.	Time to Earth's orbit.
382·32	64^{d}·5	390	12^{d}·4
382·77	33 ·2	400	8 ·53
382·99	29 ·8	450	4 ·42
383·08	28 ·8	500	3 ·28
383·21	27 ·4	750	1 ·65
		1000	1 ·16

Thus the case is reversed if the velocity of ejection exceeds 500 miles per second; it may vary enormously without much affecting T, the time of reaching the Earth. Such velocities seem high, but it must be remembered that they correspond to the case of simple gravitational attraction of the Sun, which may be considerably reduced by the repulsive force of light-pressure—to say nothing of any electrical action. The particles being small (as we know from the strong polarization of the corona), there is no reason why the Sun's attraction should not be reduced to the $\frac{1}{100}$th part of itself, in which case we must divide the velocities by 10 and multiply the times by 10. But this is probably going too far the other way. The most suitable hypothesis may be left to individual taste, remarking that if the resultant attraction is reduced to $1/m^2$ of itself, the velocities are to be divided by m and the times to the Earth multiplied by m.

FROM the *Hamburger Fremden-Blatt* of February 23 last we learn that a sum of £30,000 has been voted for the building of the new Observatory at Bergedorf, with a further sum of £15,000, spread over two years, for equipment. Dr. Schorr is a happy and, no doubt, a very busy man. We hope that he will find time to tell us something of the plans projected for the new observatory. The old one suffered a little by comparison with the magnificent "Seewarte," but it looks now as though "the boot" were not exactly to "be worn on the other leg," but to become a pair.

WILL a time come when scientific metaphor will be as freely and correctly used in the House of Commons as quotations from Horace were of old time? We have got so far as occasional attempts. We learn from the daily papers that Mr. Speaker Lowther, under cross-examination as to Parliamentary Procedure, and in particular as to "starred" and "unstarred" questions,

> lightened his opinions with a flash of wit. Mr. Blake, inclined to disagree with Mr. Lawrence Hardy as to the cause of the increase in the orally answered queries, suggested that a "very wholesome adoption" of unstarred questions was observable. "Oh," said the witness, "I think it varies a good deal with the temper of the House. If there is any electricity about the stars seem to fall rapidly."

It seems almost certain that we have here a definite attempt at scientific allusion. There is the mention of "electricity," and there is the idea of "falling stars." The next step will be to get a juxtaposition of two phenomena which are known to be more or less connected.

IT has leaked out that the appointments of the new Astronomer Royal for Scotland and of the new Royal Astronomer for Ireland each received special notice from the Royal Astronomical Society Club. The former was naturally greeted with a piper, and Scotch broth and Cock-a-leekie led up to the Haggis as a crowning feature of the entertainment. At the later ceremony the broth became "Broth of a Boy," and in place of the haggis there was "Fillet of Irish Bull." There was no piper, but Sir Robert Ball told some excellent Irish stories, and a member of the company chanted an old Irish ballad which had been recently discovered and will be found at the end of these Notes. The stories were first rate, and it is fortunately possible to reproduce one of them which has been printed in the 'Book of Trinity College (Dublin),' published on the occasion of the Tercentenary Celebration in 1891.

"THE following circumstances have been authenticated by the signatures of Provosts, Proctors, Bursars, and other College dignitaries:—In 1793 the Board ordered two of the clocks at the Observatory to be sent to Mr. Crosthwaite for repairs. Seven years later, in 1800, Mr. Crosthwaite was asked if the clocks were ready. This impatience was clearly unreasonable, for even in four years more, 1804, we find the two clocks were still in his hands. Two years later, in 1806, the Board determined to take vigorous action by asking the Bursar to call upon Crosthwaite. This evidently produced some effect, for in the following year, 1807, the Professor had no doubt that the clocks would be speedily returned. After eight years more, in 1815, one of the clocks was still being repaired, and so it was in 1816, which is the last record we have of these interesting timepieces. Astronomers are, however, accustomed to deal with such stupendous periods in their calculations that even the time taken to repair a clock seems but small in comparison."

HAVING once got hold of this 'Book of Trinity College,' which is not very widely known, it seems desirable to draw upon it further. The following references to the first occupant of the Dunsink Chair are noteworthy:—

"Ussher died in 1790. During his brief career at the Observatory, he observed eclipses, and is stated to have done other scientific work. The Minutes of the Board declare that the infant institution had already obtained celebrity by his labours, and they urge the claims of his widow to a pension on the ground that the disease from which he died had been contracted by his nightly vigils. The Board also promised a grant of 50 guineas as a help to bring out Dr. Ussher's sermons. They advanced 20 guineas to his widow towards the publication of his astronomical papers. They ordered his bust to be executed for the Observatory, and offered 'the Death of Ussher' as the subject for a prize essay; but, so far as I can find, neither the sermons nor the papers, neither the bust nor the prize essay, ever came into being."

THE election of Hamilton as Royal Astronomer is described by Sir Robert Ball in the following words:—

> On the 16th June, 1827, the undergraduate of twenty-two, William Rowan Hamilton, was unanimously elected to the Chair of Astronomy. Nor was he without formidable competitors: Airy was a candidate, and so were some of the

Fellows of Trinity College; yet a general approval, almost unanimous, ratified the choice of the Board.

Airy's own account of the matter is as follows ('Autobiography,' p. 75):—

> On April 7th (1827), Herschel wrote to me that the Professorship held by Dr. Brinkley (then appointed Bishop of Cloyne) at Dublin would be vacant, and recommended it to my notice, and sent me some introductions. I reached Dublin on April 15th, where I was received with great kindness by Dr. Brinkley and Dr. MacDonnell (afterwards Provost)..... In a few days I found that they greatly desired to appoint Hamilton if possible (they did, in fact, overcome some difficulties and appoint him in a few months) and that they would not make such an augmentation as would induce me to offer myself as a candidate, and I withdrew. I have always remembered with gratitude Dr. MacDonnell's conduct, in carefully putting me on a fair footing in this matter. I returned by Holyhead, and arrived at Birmingham on April 23rd. While waiting there and looking over some papers relating to the spherical aberration of eyepieces, in which I had been stopped some time by a geometrical difficulty, I did in the coffee-room of a hotel overcome the difficulty; and this was the foundation of a capital paper on the Spherical Aberration of Eyepieces.

But there is here no confirmation (though no contradiction either) of a legend that Airy was nearly shipwrecked on his journey to Dublin, and lost his luggage, including the letters of introduction above mentioned, and that when he presented himself in travelling garb it was very courteously pointed out to him that there was no proof of his actually being the person he represented himself to be. Whereupon he replied that if his interlocutor would only set him one or two stiff mathematical problems, he would endeavour to show that at any rate he was one of a limited number of persons.

THE text of the old Irish ballad has been kindly communicated to me. The date and author are not specified, but the crude attempts at rhyme, culminating in the practical confession of failure in the last line but one, suggest a very early origin. The only difficulty is the mention of Father O'Flynn (1), which would seem to indicate a date subsequent to that of this well-known poem; but the doubts thus suggested are set at rest by the occurrence of the line marked (2), which has clearly been borrowed in the poem of Father O'Flynn from this old ballad. Another instance of plagiarism, though in this case only in general idea, is afforded by the use of the word "glass" (3) in the second line of the chorus. An ambiguity is apparently intended, the word referring partly to a telescope and partly to a drinking-vessel; and this quaint conceit has been adopted in the "Astronomer's Drinking Song," sung in 1798 ('Budget of Paradoxes,' pp. 234-5).

Whoe'er would search the starry sky
  Its secrets to divine, sir,
Should take his glass—I mean, should try
  A glass or two of wine, sir!

The author of this song, one would think, must have been acquainted with the ballad given below.

There is apparently some confusion between stars and planets (4), which is either due to the imperfect information of those early days, or may be intended as an indication of the post-prandial character of the ballad; indeed, it would not be perhaps too strong to call it bacchanalian. We may either attribute this to the predilections of the unknown author, or, as has been ingeniously suggested, to the fact that one at least of the old astronomers mentioned in the last verse is known to have been a staunch advocate for temperance reform; and this explains, by the rule of contrary, the personal allusion marked (5). But if this view be correct, we must credit the author with a remarkably subtle sense of humour considering the time at which he wrote. Following out this idea, a German commentator has suggested that "wink" (6) in the last line of the first verse is a corrupt reading for "drink," but this view is not supported by the best available MS.

Old Irish Ballad.

I. Gintlemin all, if ye kindly will honour me,
Lind me your ears, widout anny economy:
Troth! and I'll sing ye the praise of Asthronomy,
Takin' the chune from ould Father O'Flynn (1).
Wance in a way we're neglectin' the shtars,
Thransits of Vanus and markin's on Mars;
"Is it lave gaiety all to the laity?" (2)—
Cannot asthronomers wink (6) now and thin?

Chorus.—Thin, let us be glad we're asthronomers born,
Handling a glass (3) from the dusk to the dawn:
All observations and all computations
And *in*-vestigations can wait till the morn.

II. Watchin' the stars, 'tis a fine occupation, al-
Tho' ye may see nothing very sensational;
Aisy they moves under force gravitational (4)—
"Arrah! be aisy"'s a jool of a text.
See how each wan of thim houlds to its place,
Sidling away p'r'aps the ghost of a trace:
"No separation!" cries each constellation—
So miss the last train, and we'll wait for the next.

III. Some will be praisin' the use of geography,
Give me the pleasures of lunar topography:
If ye prefer it we'll say "Selenography"—
Annyway, lookin' the Moon in the face.
Sometimes she's fat and thin next she'll be thin,
But ivery month she comes back wance agin:
The Moon, dacint craychur, tries this way to tache yer
Just ivery month to be here in yer place.

IV. Good men are scarce, and 'tis sad when we're losin' 'em,
Others to fill up the gaps they'll be chusin' 'em;
Good men they'll want, and we can't be refusin' 'em,
Though 'twas MacDyson they tuk the last time.
Now they've got just the right man for Dunsink,
Quick at the sums, and the divil to drink! (5)
Here's to O'Whittaker, tho' not a bit a co-
Nvaynient name for to fit wid a rhyme.

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THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Wednesday, 1906 April 11.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: E. T. WHITTAKER, M.A., F.R.S., Andrews Professor, and T. LEWIS.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Lewis. Ninety-four presents have been received since the last Meeting. Among them will be found a volume from the Royal Observatory, Greenwich, entitled 'Determinations of Longitude 1888–1902'; Volume III. of the 'Lowell Observatory Annals,' presented by Mr. Percival Lowell; 'Report of the U.S. Government Surveys,' by Major-Gen. Waterhouse, presented by the Author; and G. W. Hill's 'Collected Mathematical Papers,' Volume II., presented by the Author. We have also received six large transparencies from photographs taken by Prof. Barnard at the Mount Wilson Observatory, California, presented by the Yerkes Observatory, and eighteen enlargements of Astrographic Chart plates from the Royal Observatory, Greenwich, in continuation of the series partly received.

A vote of thanks was accorded to the donors, and the *President* called on Mr. Cowell to begin the proceedings by explaining the paper presented by him.

Mr. Cowell. I have recently shown that the apparent motions of the Sun and Moon seem both to be subject to accelerations of $4''$ and $5''$ respectively.

Tidal friction is capable of accounting for the latter. It has been assumed that it can only account for the same proportional increase in the Sun's motion—that is to say, as there are thirteen months in the year, the secular acceleration of the Sun mu necessarily be one-thirteenth of that for the Moon.

At Mr. Eddington's suggestion I have tried to apply Sir Geor Darwin's ideas to this subject. It led to the following explanati of the above phenomena. Let it be assumed that tidal frictio increases every century the length of the day by $0^{s}\cdot005$. The then will be an apparent secular acceleration of 4" for the Su and at first sight it would appear to be 50" for the Moon; b of these 50" a great part must be knocked off, for Sir Geor Darwin has shown that an increase in the length of the day mu by the principle of conservation of angular momentum, be acco panied by an increase of the Moon's orbit and diminution of i orbital motion. I am unable to calculate exactly by how mu the 50" must be diminished on this account, but a rough estima leads to the value 37". If 45" be the true quantity, then shall be left with an apparent secular acceleration of 5" for t Moon.

My point is that 37" and 45" are quantities of the same ord of magnitude, and that 45" is not inconsistent with possi values of other unknown quantities in the equation of angu momentum.

The Astronomer Royal. This seems to be a very important a interesting suggestion, which appears to complete the w Mr. Cowell has done as regards the discussion of Ancient Eclips He has put it forward very modestly as a suggestion; but it see to me to carry weight, and is an important result in itself, fitt in with Sir George Darwin's work on the effect of the tides. wish to offer my congratulations to Mr. Cowell on this brilli piece of work.

Prof. Turner. I should like to add my congratulations also those of the Astronomer Royal. Mr. Cowell's papers succeed another so rapidly that they almost take one's breath away. we have had at least one month or more, if I remember right to consider the problem and the tentative solution which he now put forward. We have all probably spent some mome pondering over what he placed before us some weeks ago—t there was apparently some cause which had a large effect for Earth, but the effect for Mercury was zero. What is reason of the discrepancy? The natural supposition would be Mercury to suffer a greater effect than that found for the Eart and yet the discussions of the transits of Mercury showed contrary. One suggestion that an eminent physicist made hearing of the situation was that the solution might possibly lie something magnetic. We know the Earth is a magnet; but have no evidence that Mercury is a magnet; and he sketched a theory I need not dwell upon, on which there might be effect of this kind for the Earth which was zero for Mercu But Mr. Cowell has furnished a much simpler explanation making use of the obvious fact that the thing which we have and which Mercury has not got, is a satellite, and he has given us a beautiful solution of the problem *. We only trust that so elegant a solution may be confirmed.

Prof. Sampson. I have been struck with what Mr. Cowell has said. But I would like to ask him if he could throw light upon the process by which this very large secular accleration of 37″ or 45″ has been apparently ignored by astronomers up to the present. Was it due to a state of suspense as to the truth of Sir George Darwin's theory? Or have they written off that amount as a kind of bad debt? All this certainly ought to be accounted for. From what Mr. Cowell has said we ought to have some explanation either in support or depreciation of his theory of the secular acceleration of 40″ or more in the Moon, whereas we have been in the habit of considering it was only 5″ that we had to account for. By what process have we shut our eyes to the existence of that acceleration? Why has it not come forward as an outstanding question, considering that the Moon's theory has been so constantly and carefully surveyed by astronomers for a long time past?

Mr. Cowell then repeated certain of his explanations.

Prof. Sampson, continuing, said:—That is a repetition of what has been said before, and it was, and is, perfectly clear. But what puzzles me is how astronomers have managed in the past to ignore this consequence of Prof. Darwin's theory.

Prof. Turner. You have made a certain calculation, Mr. Cowell, and got 37″. Well, Prof. Sampson wants to know why nobody made that calculation before? (Laughter.)

Mr. Cowell. When Laplace or Lagrange suggested that the length of the day might possibly be changing, I do not think that they realized such a change must react on the Moon's orbit. When Prof. Darwin traced out his theories of the Moon's evolution, I think he was more interested in the question of evolution—what happened 57 millions years ago—than in the bearings of the subject on modern astronomy—on what happened 2000 years ago. This leaves Prof. Newcomb to be accounted for. In answer to Prof. Sampson's question, I can only say that I suppose Prof. Newcomb made an oversight.

Mr. Crommelin. I should like to join my congratulations with those of others to Mr. Cowell on his most interesting result. One of the most interesting things about it is that it shows for the first time the Darwinian effect of tidal retardation and lunar recession going on visibly under our eyes. Few of us doubted that what Darwin said was going on. But there is a great difference between believing a thing and seeing it: just as I think everyone felt great satisfaction when the spectroscope showed unmistakably that Saturn's rings were not a solid continuous surface, but were a multitude of tiny bodies, although that had been mathematically proved many years ago. This is the sort of result that carries conviction with it and shows this action is really going on. The fact that the Sun and the Moon both show it in approximately the right degree makes it pretty clear that it is the real explanation of a real action, and that the retardation of the Earth's rotation has been established by observation. It is true that before this time people have put down a part of the lunar secular acceleration to that retardation; but none of them had got the exact observed value of the secular accelaration of the Moon. There were very varying results got; and only a few months ago Mr. Nevill claimed that the ancient lunar eclipses were explained by using the Laplace term alone without any question of tidal retardation whatever. Mr. Cowell has shown the amount of tidal retardation and that it is much greater than was thought possible. So I think all discussion of ancient astronomical observations will have to be revised in the light of this hypothesis.

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* [These words were spoken on the spur of the moment and involve a misconception. The lengthening of the day should of course affect Mercury more than the Earth. Mr. Cowell informs me that this difficulty is left outstanding. —H. H. T.]

Mr. Stratton. The 50″ depends, I presume, on some arbitrary choice for the viscosity of the tides, viscosity of the water of the ocean; and that was chosen, I suppose, to give a satisfactory figure for the retardation of the Earth. If the effect had not been increased 10 times there would have been no need for a second term to be brought in; and therefore I presume that was why it was not looked for before. Prof. Darwin's theory left the exact amount of this tidal retardation absolutely vague, as far as I know, and Mr. Cowell has shown that by increasing it beyond the amount assumed in the past, one has found an explanation for the terms hitherto not suspected.

Mr. Hinks. Will the great alteration in the tidal effect upon the rotation of the Earth require us to modify our views as to the internal rigidity of the Earth?

The Astronomer Royal. Would the 37″ have been increased to 45″ by another assumption as to the secular change of the eccentricity?

Mr. Cowell, in response to the Astronomer Royal, wrote on the board the equation of conservation of angular momentum. He continued:—I am forced to represent by symbols two quantities that are not accurately known, the moment of inertia of the Earth and the rate of change of the eccentricity of the Moon's orbit. The 37″ was derived on the supposition that the Earth is homogeneous and that the eccentricity of the Moon's orbit is not changing owing to tidal friction. If the Earth is not homogeneous, it is reasonable to suppose that its denser parts are nearer the centre, in which case the 37″ requires to be diminished. On the other hand, the 37″ can be brought up to 40″ as required, if it be supposed that the eccentricity of the Moon's orbit is increasing at the rate of 0''·01 per century. Modern observations at any rate do not contradict this supposition.

Thanks were given to Mr. Cowell, and the President called on Mr. J. M. Stratton to read his paper on "Planetary Inversions."

Mr. Stratton. When Prof. W. H. Pickering discovered that the orbit of the newly-discovered ninth satellite of Saturn, Phœbe, was retrograde, he offered as an explanation his theory of planetary inversion; he suggested that originally the planets rotated in a retrograde direction, but that under the action of the tides they had tilted over until at any rate the inner planets rotated in the opposite direction; Phœbe, the first satellite of Saturn to be born, had been left behind, while the inner satellites had followed the planet's equator over. My paper is an attempt to examine this theory in the light of the theory of tidal friction developed by Sir George Darwin in a series of papers about 1880. The first part extends Sir George Darwin's results for the secular changes, caused by solar tides, in the obliquity of a planet unattended by a satellite; it shows that if a planet is originally rotating in a retrograde direction, that is with an obliquity greater than 90°, it will tilt over until its obliquity lies somewhere between 90° and 0°; the exact equilibrium value for the obliquity depends on the rates of rotation and revolution of the planet and on the viscosity of the tidally-disturbed layers. So far these results support Prof. Pickering's view, but complications are introduced with the presence of satellites, such as exist in the solar system, whose tidal influence on their primary is large compared with that of the Sun. While the tidal effect on the inclinations of the satellites is negligible—other secular terms entirely obscuring the small tidal terms,—the planet's obliquity, affected by no other large secular terms, may be considerably altered by the satellites' tides: so much so that in the presence of such satellites the obliquity of a planet has three equilibrium values, near 0°, 90°, and 180°. If, as is the case with Jupiter, Saturn, and Uranus, there be a group of satellites no one of which is much more tidally effective than the others, then the second position is unstable and the planet will be driven away from it towards an obliquity of 0° or 180°; in the presence of one satellite and under special circumstances the stable value of the obliquity will be near 90° and the planet will ultimately move with its axis of rotation in the plane of its orbit. The suggestions as to the past history of the solar system offered by the theory are as follows:—Mercury, Venus, the Earth (prior to the evolution of the Moon), and Mars have all, under the influence of solar-tidal friction, tilted over from an initial position with retrograde rotation into one with direct rotation. Jupiter's obliquity decreased below 90°, under the influence of the Sun, before it evolved its satellites. Partly under their influence its obliquity has decreased to a very small value. Saturn certainly evolved Phœbe, and possibly also Uranus and Neptune, before its obliquity had decreased below 90° under solar-tidal friction. As it passed through the critical value of 90°, Phœbe, the farthest removed of its satellites, fell back into the plane of the planet's orbit, Iapetus and Hyperion fell into the plane of the planet's equator and followed it over. Later on the other satellites and the ring were evolved and under their influence Saturn's obliquity has steadily decreased and is still decreasing. Uranus and Neptune evolved their satellites before their obliquity decreased to 90°, and they are now being driven back by them towards an obliquity of nearly 180°.

With regard to this application of the theory, I must mention that, from such examination of the question as I have so far made, it seems that the orbits of the satellites will increase in size very rapidly compared with the rate at which the planet's obliquity alters. If this is so, then unless the satellites go through an annular stage in the course of their evolution their effect on the planet's obliquity may be only very transient and they will be driven too far away from the planet to be tidally effective before making any appreciable difference in the planet's obliquity. This theory certainly suggests, though it does not absolutely require, an annular form in the past history of the satellites of Jupiter and Saturn. This is one difficulty which faces the theory: other difficulties, which it seems almost impossible to meet properly, are the enormous amount of time involved—time long enough, perhaps, to invalidate the equations used—and the effect of heterogeneity and compressibility in the planets. These difficulties though they render acceptance of the theory hard are not necessarily destructive of it. Such would have been the case with a criticism made by Prof. Moulton in the 'Astrophysical Journal' recently, had it not been invalidated by a misconception of the nature of Prof. Pickering's theory. With regard to this, perhaps I might say that though one may be stimulated by Prof. Pickering's persevering and successful search for Phœbe, by his bold and equally successful idea of a retrograde orbit for it, and by his suggested explanation of this orbit, one must take care not to be misled, as Prof. Moulton was, by the vague language employed by him in outlining his theory. In conclusion, I may add that the theory remains a speculative hypothesis, on the whole well supported by the theory of tidal friction, and the only one yet offered to explain the existence of certain facts.

Prof. Turner. It is a remarkable testimony to the vitality of Sir George Darwin's work that we should have two papers of this kind at the same Meeting, both having their roots in that work. I am very much struck with the rate of advance in astronomical novelties nowadays. It was only in 1904 that I heard from Prof. Pickering, on arriving at Harvard, of the re-discovery of Phœbe, and of this extraordinary fact of her going round the wrong way. At the same time he told me of his idea of the original rotation of Saturn having been retrograde and, roughly speaking, of the hypothesis with which Mr. Stratton has been dealing. It is extraordinarily interesting and satisfactory to find that this original idea on the part of the discoverer of Phœbe should be confirmed, at any rate to some evtent, by Mr. Stratton's work. To use a phrase that fell from Mr. Cowell, I think people will find it hard to deny the hypothesis now. As Mr. Stratton said, the whole subject is extremely speculative, and I may add very difficult; and it does immense credit to him to have grappled with the large amount of numerical work and with the many other difficulties as he has done in the paper, which, by the way, I have had the pleasure of seeing; and having seen it I am not at all surprised that it was honoured with the Smith's Prize at Cambridge, as it was recently. He did, perhaps, less than justice to himself in himself making so little of the difficulties. He might have taken the striking plums out of the paper, such as the very definite result given about Uranus: the obliquity of Uranus's equator is only 97°, and Mr. Stratton's work suggests that the satellites must have been born when the obliquity lay between this value and 98°. Now these limits are so narrow as to give us a most striking and almost sensational idea of the dates when the satellites of Uranus were born. Then there are other very interesting points in the paper, which I am sure will be read with great interest by the Fellows when they see it in print.

The President. There is no reason for Mr. Stratton to apologize for the speculative character of his paper. It is only by papers of a speculative character that we can possibly arrive at a satisfactory theory to account for the facts that are observed. I am glad Prof. Turner has mentioned that he has been awarded the Smith's Prize for the work, and we congratulate Mr. Stratton upon this success. I will now ask you to return a vote of thanks to him for his paper.

Mr. Crommelin. In adding my congratulations to those already uttered I may perhaps say that I made the suggestion about a year ago in the Council Report on Phœbe that this research should be taken up. I was not competent to do it myself; but I felt that Prof. Pickering's able hypothesis was one which promised so much in explanation of the peculiarity of the satellites of Saturn that it ought to be taken up mathematically. I am glad to find that the mathematical explanation is a sound one. Of course one cannot follow it all when put in a brief way on the screen; but as far as one can follow one sees that there are evidences that it will explain the observed facts. I do not think the two new satellites of Jupiter were mentioned. They have puzzled me very much; yet these two must be ancient satellites. What is Mr. Stratton's theory—are they in a stable stage, or are they approaching the plane of the orbit or are they receding from it?

Mr. Hinks. I am sure if Sir Robert Ball had been present he would have expressed his own gratification and said how gratified the rest of us at Cambridge were that Mr. Stratton, during the time of his Studentship in the University, elected to throw in his lot with the Cambridge Observatory and spent his time at the Observatory working at the things he has been telling us about to-day, as well as in more definitely observational work. It is the first time that an Isaac Newton Student has been upon the Observatory staff during his studentship, and I am sure the rest of the staff congratulate him on this research.

Mr. W. W. Bryant. Is it not the fact that the possibility of an annular stage has been utterly discredited, because there is no reason whatever why the annulus should concentrate at any particular point of the ring?

Mr. Stratton. Prof. Pickering's latest remarks on his theory suggested that he had obtained the explanation of the large inclination of the two new satellites of Jupiter in that they had been left behind as Jupiter tilted over. I do not think that is the explanation. The planes of the satellites' orbits move so rapidly compared with the planet's equator that we may regard the satellites as always oscillating about the equilibrium position corresponding to the actual obliquity of the planet, and it does not seem correct to speak of a satellite as being left behind. The direction of the motion of Jupiter VI. and VII. merely indicates that they were evolved before Jupiter's obliquity had decreased below 90°.

With regard to the ring question, certainly Sir George Darwin has said that it seems almost impossible for a ring to develop into a satellite. I should add that the ringed stage for the satellites of Jupiter and Saturn is not absolutely required for Prof. Pickering's theory.

Prof. Fowler read a paper on "High-level Chromospheric Lines and their Behaviour in Sun-spot Spectra," in which he extended the comparison of the chromospheric and spot spectra mentioned on a previous occasion. When observing the spectrum of a metallic prominence with a tangential slit, many of the bright lines are very short and most likely represent the flash-stratum, while others are relatively long and apparently represent the higher layers of the chromosphere. The low-level lines correspond with lines found in the arc spectra of the metals, while many of the high-level lines have been identified as enhanced lines of iron, titanium, and chromium.

In the spot-spectrum some of the Fraunhofer lines are intensified and some are reduced in intensity. The intensified lines, if observed in the chromosphere at all, are low-level lines, while many of the enfeebled lines agree in position with high-level chromospheric lines. Excluding the lines of hydrogen, helium, sodium, magnesium, and barium, thirty of the chromospheric lines recorded are believed to be of the high-level type, and there is evidence that each of these is weakened in the spots. Confirmatory evidence as to this behaviour in spots is afforded in many cases by the observations of Mitchell, and by the recent photographic records of Hale and Adams.

It is probable that the vapours producing the high-level lines and the corresponding dark lines in the solar spectrum are chiefly restricted to the upper parts of the chromosphere, but it is not yet clear whether the weakening of the lines in spots is due to the actual withdrawal of those vapours, or to the umbra not providing a sufficiently luminous background to strongly exhibit their absorption. The most definite conclusion indicated by the observations is that the enhanced lines of iron, titanium, and chromium appear as high-level lines in the chromosphere and are reduced in intensity in the spectra of spots.

Prof. Sampson. Under such circumstances as those in which I now find myself placed, it is perhaps unusual to make a communication to the Society. I am engaged in making a discussion of the observations of the eclipses of Jupiter's satellites made at Harvard Observatory, but although this is not completed I have arrived at some interesting points in a problem which is old and somewhat famous, and so I think I may have your permission to tell you some of them. Though I want to cut as short as possible what I have to say, yet it will make what follows more clear if I say something as to the history of this problem. It arose in the middle of the 18th century at a time when the eclipses of Jupiter's satellites were in competition with the Moon and the chronometer as to the best means of determining longitude; and it may be remembered that one of Sir Christopher Wren's anagrams on the method of determining longitude seems to make obscure reference to a method of observing the eclipse of the satellites. Be that as it may, they were with the Earth's Moon the only extra-terrestrial clocks which were available for that purpose, and from that point of view they possess great and almost unique interest. The facts soon showed that it is impossible to observe these satellites with any profit until their theory had been fathomed. The theory proved to be difficult. But you must not suppose the old investigations were by any means all bad. As I shall have to refer to some of them again, I will copy down and bring up to a common date 1750 the following determinations in the past of the position of the fourth satellite. Satellite 4, as you know, is a very difficult satellite to observe with any precision, so I think this comparison is very creditable to the old astronomers who reduced this somewhat vague material. An important one is left out, but I have not the material yet to reduce it. But that is enough to show that we ought to attach considerable credit to the old reductions, and especially to the determination made by Delambre, which we know was based upon a large amount of material and carried out with exhaustive care. But although these were good, no satisfactory theory was supplied till Laplace and Lalande took the matter in hand, and they showed that the theory of satellites was one of the most varied and beautiful parts of the 'Mécanique Céleste,' and Laplace in particular gave as usual the full numerical theory in addition to the mathematical expressions, relying for his reduction upon the constant determined by Delambre. Delambre must have been a man of immense energy and power of research. Besides the great histories by which he is best known he made two separate calculations of the tables of the satellites. The second of them was based upon all the eclipses which he could collect from the first observations of 1660 down to 1808. The other was from his extensive correspondence, and they make his collection of observations much more complete than anyone could attempt to make at the present day; they amounted to more than 7000 in number; and it comes out as one result of my present work that his determinations in general process have a high degree of authority. His tables were all of short duration; they terminated in 1840 and 1836. Then the tables brought out by Damoiseau, which expired in 1860, but which have been carried on by one shift or another for now four decades since, were used. Damoiseau's tables are in some respect more modern than those of Delambre. But almost from the date of their publication they excited considerable distrust. In 1842 it was shown that there were inconsistencies among them. In fact, in the introduction to the Tables there are admitted inconsistencies as well as incompletenesses. And there is a further point which will strike, I think, anybody who knows how much astronomy of this character has to go upon good faith of the author, that Damoiseau adopted wholesale all the constants relating to the first satellite from Delambre without anywhere mentioning the fact. That is a thing which excites the greatest suspicion in all his work: one does not know what lies behind what he says. It may excite surprise that tables so absolutely flawed and condemned like Damoiseau's should have been allowed to serve so long and should have been even prolonged up to the present day. But the explanation is not obscure. In the first place, the theory of the satellites, although it can never drop out, does not stand in the way of bringing the other parts to perfection; it stands by itself, and it was not the most pressing problem; there were greater ones. Then the analytical theory was reputed to be exceptionally difficult; in fact its reputation was worse than was borne out by facts. But surely the reason that overweighed any other, if we examine the matter, and there must have been many who have looked carefully into the question to see whether these satellite theories could be brought into better order, will be found to be that the observations were of an incurably vague character: it would seem doubtful whether anything that one could bring to bear in the way of modern treatment and reduction could materially improve what had been done by Delambre and Damoiseau, both of whom it is clear, from the language of the introductions to their tables, were somewhat disappointed with the fruit of their labours. Well these obstacles, such as they are, have disappeared one by one. The neglected state of this theory has only become more prominent as the other parts were carried to greater perfection, and researches such as those by Dr. de Sitter and Prof. Kapteyn have been made. But, as you observe, nothing was done towards improving the observations until the series of which I am now to give you a description was undertaken in 1878 at Harvard and carried on without intermission and by the same observer (Prof. Wendell) up to the present time; and these observations made a final clearance of all that was vague in the old observations. The eclipses are now observed with a photometer, and what is really observed is an increase or decrease of magnitude as time goes on. At some point the satellite is lost to view. Where it is lost depends upon the telescope, upon the observer, even upon the Sun's elevation and that of Jupiter and other things. It depends upon a host of causes which can only be treated in a summary way, and the result is an extremely unsatisfactory statement as to where upon this curve the satellite is lost sight of. Prof. Pickering treated that in the only possible way by observing the whole phenomenon; he takes time observations of magnitude all the way along here, and we get a number of observations spaced out in proper time and proper magnitude showing a light-curve throughout the whole eclipse. That is the obvious and only scientific way of treating it, and it settles all that is vague about observations. Each observation of the satellite will consist of from 20 to 40 times and measures of magnitude. Some 700 eclipses observed in this way have been brought under discussion, a half of which refer to the first satellite: and the total number of measures of magnitudes, excluding those which are taken as a check before the eclipse really begins, amounts to upwards of 15,000, these being more than double the observations upon which Delambre built. I shall enter but little into details of the further discussion, because it is difficult to make it clear, and if it is not clear then it immediately becomes tedious. But there are a few points that I would like to mention to you. The first is the theoretical discussion of the light-curve of an eclipse. The satellite gradually passes into the shadow, and you have to allow for the diameter of the Sun and the satellite and other features before you find the speed at which the eclipse takes place; and, on the other hand, if you have to determine the normal light-curve of the eclipse you can infer from that the diameters of the satellites, and you actually get by that a determination of the diameters of all four satellites which, in my opinion, are more reliable than determinations by the micrometer. After the whole work was completed Prof. Pickering wrote to tell me that he had discovered that in the middle of the series the adopted longitude of Washington has been changed by four seconds, affecting through certain curves the reference point of his observation to the ephemeris. Of course discontinuity like this could not be tolerated, although to remove it was to involve, as you know, a vast amount of additional reckoning. However, it was a satisfaction to Prof. Pickering that I had discovered and recognized that changed longitude. My computations indicated that the longitude of Washington was changed not once but five times, although, for the most part, by an almost insensible amount. Altogether as a result of revising the theory of the Tables some 15 or 20 separate corrections had to be computed and applied to each eclipse, and that work was of considerable length on account of the small assistance that I had at hand. I would like to add a word as to the unknowns for which equations were solved. The most interesting is the equation of light. It was through the satellites that the equation of light was first discovered, as everybody knows; and these satellites are the sole place in the whole of astronomy in which the equation of light asserts itself in a way that leads to any reasonable sound determination of the solar parallax. I shall give you shortly the values I got. In whatever case it was possible I determined the same quantity independently from the observations of different satellites as a check upon the general consistency, a check which, in my opinion, is far more significant than the determination of probable errors, though, of course, I give that also: a very interesting constant that comes out in connection with them is libration of the first three satellites. But I need not explain that at the present moment. The old observations quite failed to determine it. A very peculiar difficulty showed itself in connection with the eccentricities of the first and second satellites: these are accompanied by an inequality, the character of which is actually of greater magnitude than the eccentricities to which they give rise.

The value I got for equation of light from my own reduction of the observations is that which gives 8″·799 with a probable error of $\pm$0″·022. Of course the probable error rather spoils it. It puts it in the second rank as a determination of solar parallax; but the result is very satisfactory, because it reflects a certain amount of credit upon other determinations which may be sound, although the large residuals have nothing to do with, in fact are quite independent of, the quantity which you profess to be determining. All the eclipses were read separately for determination of time by myself and at Harvard, and I have separately reduced the whole set, so I have double determinations of everything.

As it was now past 7 o'clock, Prof. Sampson had to postpone the remainder of his address.

The following papers were announced and partly read:—

Rev. W. Hall. "Note on the Effects of Differences of Sea and Air Temperatures on Marine Refraction."

E. E. Burnard. "Observations of the Magnitude and Position of Nova Geminorum."

Sir C. Todd. "The Partial Eclipse of the Sun, 1906, February 22, observed at Adelaide."

F. J. M. Stratton. "On Planetary Inversion."

A. Fowler. "High-level Chromospheric Lines and their Behaviour in Sun-spot Spectra."

P. H. Cowell. "Explanation of the apparent Acceleration of the Earth's Orbital Motion."

Royal Observatory, Greenwich. "Observations of Comet *a*, 1904, from Photographs."

E. M. Antoniadi. "On the Part played by Contrast in the Appearance of the Dusky Areas of Mars."

The following gentlemen were elected Fellows of the Society:—

Rev. W. Briscombe, Hillam Royd, Abbey Park Road, Grimsby.
A. S. Eddington, B.A., Royal Observatory, Greenwich, S.E.
Dr. Edalji Modi, F.C.S., Sleater Road, Bombay.

The following Candidates were proposed for election as Fellows of the Society:—

John de Fenton, Seymour Avenue, Parktown, Johannesburg (proposed by R. T. A. Innes).
John Grigg, Thames, New Zealand (proposed by W. S. Aldis).
Robt. Leetham Jones, M.A., 3 King's Bench Walk, Temple, E.C. (proposed by F. J. W. Crowe).

The following foreign astronomer was proposed by the Council as an Associate of the Society:—

Julius Franz, Observatory, Breslau.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1906 March 28.

A. C. D. Crommelin, B.A., *President*, in the Chair.

Secretaries: J. A. Hardcastle.
J. G. Petrie.

The Minutes of the previous Meeting were read and confirmed.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of seven new Members was unanimously confirmed.

Dr. Downing, in a paper entitled "Planetary Conjunctions in May, 1906," drew attention to the approach of Venus and Mars on May 6, Venus and Jupiter on May 11, and Mars and Jupiter on May 18.

Mr. Hardcastle read a paper on the "Positions of the Planets in February," in which he pointed out that on February 23rd of this year the following heavenly bodies were as near as possible in a straight line: the Earth, the Moon, the Sun, Mercury, Venus, and Saturn. Not only were they practically in conjunction in R.A. but also within two degrees of Dec.

The President said that both these papers were of considerable interest—the one dealing with a series of future conjunctions and the other with a past conjunction. The conjunction in February was remarkably close, and the whole of the five bodies concerned were within about two degrees of Right Ascension and one degree of Declination, so that it was certainly a conjunction which the ancient astronomers would have thought a very significant one. He hoped Members would take advantage of Dr. Downing's note and look out for the conjunctions that were coming on in May. They would be beautiful and attractive spectacles, and they also, as Dr. Downing had said, gave the possibility of comparing the luminosity and colour of these three planets.

Mr. Seabroke exhibited a slide of Saturn made from a drawing by *Mr. Merlin*, a Member of the Association residing at Volo, showing three very faint dusky spots or patches on Saturn's bright equatorial region. It struck him that very considerable detail existed on the bright equatorial region of the planet and that under still better atmospheric conditions much more could have been seen.

The President hoped that there would be a large number of observers who would do their best to look out for spots that were available for the purpose of determining the rotation period of the planet.

A note by *Mr. P. M. Marshall* on "The Visibility of Mercury" was next read.

Mr. Hardcastle said he saw it himself on the 7th of March and it was seen again on the 14th and 17th. He was surprised at seeing it on the 7th; in fact, at first he imagined it was Venus, though, of course, a moment's calculation showed that it could not be Venus. He doubted whether the story of Copernicus was correct. It certainly seemed rather difficult to believe that he had never seen the planet.

Mr. Lynn said that some years ago he pointed out that the assertion found in many books that Copernicus had never seen Mercury was quite untenable. There was really no evidence for the statement.

Mr. G. F. Chambers said it did not seem to him that Mr. Lynn's statement was at all a convincing one. So far as his memory served him, Copernicus had himself left it on record that he had never seen the planet, and it did not seem to him that that testimony was overthrown by a gentleman who said he ought to have seen it.

Mr. Lynn replied that the answer to that was simply to ask for reference to the chapter and verse where Copernicus made the statement; he for his part could not find any such.

The President said that as the word "observation" was in question, the point was whether it referred to an astronomical observation of position, or whether it merely referred to an ordinary view of the planet.

Mr. Chambers said that, considering the date at which Copernicus lived he did not quite see that he could have done anything else but simply look at Mercury if he had the chance.

Mr. Goodacre gave an abstract of a paper on "Selenographical Studies," by *Mr. Victor Nielsen*, of the Urania Observatory, Copenhagen, and stated that it had been translated from the German by Mr. Chambers. In 1878 Dr. Klein had startled the astronomical world by stating that he had discovered a new formation on the Moon's surface near Hyginus. The formation which he called attention to is now known as Hyginus N. Mr. Goodacre said that the most important point in Mr. Nielson's paper was the statement that since the discovery of Hyginus N. other changes had been taking place in its immediate neighbourhood. Mr. Nielson's sketch showed two small dark spots which he had found close to N. These objects were found also on drawings by Krieger and Leo Brenner made in 1904, and as they do not appear on drawings made in 1878 by Green and Neison, when this district was carefully scrutinized by them, it is difficult to escape the conclusion that they are two new formations.

Mr. S. A. Saunder wished to say a word regarding the rill in Ptolemæus. He had seen the rill several times; in fact the drawing was made primarily from his own observations. Col. Watson, of Camberley, had also seen the rill extending past the crater numbered 7 and terminating nearly at that numbered 18. Although a great deal of time had been spent on Ptolemæus, he felt sure that there was a great deal to be discovered in it yet and a great deal more round the walls.

Mr. Goodacre said he noticed that the whole surface of the floor was covered with irregularities at sunrise, and quite different from the smooth lagoon-like appearance as seen under a higher light.

Mr. Lynn remarked that the first man to apply the word "rills" to the formations under notice was the late Julius Schmidt, of Athens; but the word in English suggested a different meaning from what he intended by it, the German word "Rille" signifying a furrow.

In answer to a question *Mr. Goodacre* said the Schröter valley, one of the rills visible with a small telescope, was 2 to 2½ miles in its widest parts. Many of the river-beds referred to by Prof. Pickering were from 1000 to 2000 feet in width, and required considerable optical power and good atmosphere in order to see them at all.

Mr. Hardcastle said one could not see to measure on a photograph craters that came to much under two miles in diameter.

The President said their thanks were due to Mr. Nielsen for his paper and also to Mr. Goodacre for the trouble he had taken in illustrating the paper by slides.

Mr. Maunder gave particulars of two new comets discovered since their last meeting. Comet 1906 *b* was discovered on March 3 by Dr. Kopff. It was moving exceedingly slowly at discovery, and its small apparent size and definite outline marked it out from the first as an exceptional member of its order. It was moving very nearly in the ecliptic and was almost exactly in opposition to the Sun. The orbit was remarkable for its very great perihelion distance, viz., 3·1 times the mean distance of the Earth, or 289 millions of miles. Comet 1906 *c* was discovered by Mr. David Ross, of Melbourne, on March 18. Mr. Ross is a Member of the Association and Hon. Secretary of the Victoria Branch. The principal feature of Comet Ross is its high inclination, 80°. The Members of the Association in the southern hemisphere have been specially active in this field of work. Mr. John Tebbutt has discovered five comets, Mr. Walter Gale one, Mr. Davidson one, Mr. John Grigg three, and now Mr. Ross has secured his second, making 12 in all.

The President said that he had been very hopeful that this comet of Kopff's was going to turn out a great comet. From its slow motion for some days he drew the conclusion that it was probably a long way off, and in that he was right. But he had also hoped that it was coming up to perihelion instead of having passed it. It was curious that we had had an epidemic of comets of great perihelion distance and probably many small ones passed through perihelion unobserved by us.

Mr. Lynn read a paper on "Halley's Comet and Zadkiel," and a paper by *Mr. E. M. Antoniadi,* on the "Light which spread over Constantinople at night before the Fall in 1453," was read.

Mr. Hardcastle read an interim report of the Variable Star Section under the title of "A Study of Algol," by *Col. E. E. Markwick, C.B.*

The President said he was very glad the Section had taken up this, perhaps the most interesting of all variable stars, and one that was within the reach of almost everybody. Certainly there was much to be learned from a careful study of the light-curve by seeing the character, especially at the beginning and end of the eclipse and at the period of minimum. He thought that Dr. Vogel found he could explain the curve only by supposing extensive atmospheres for the stars; the curve at the beginning and the end of eclipse should be examined to test this point.

Mr. Saunder having taken the Chair *pro tem., The President* exhibited and explained a diagram illustrating a transit of the Earth and Moon over the Sun as seen from Mars, 8th May, 1905. There would not be another phenomenon of the kind till 1984. The only observable result of the transit was that at that time the illumination around the disc of Mars would be practically uniform, so that it was a very favourable time for making measures of polar compression, because there would be no allowance to be made for defective illumination, which was always an uncertainty with a planet with an atmosphere.

The Meeting then adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday evening, April 18th, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. R. Bentley, F.S.A.*, President, in the Chair.

Mr. Alfred Hands read a paper on "Some so-called Vagaries of Lightning reproduced experimentally." He said that lightning is an electric discharge, and as such it should act in accordance with the laws that are known to govern the subject. We have doubtless much to learn yet as regards electricity, but even though fresh facts and laws may be discovered they can scarcely be in direct opposition to those already known, and therefore if an occurrence does not appear to accord with our knowledge we should try and fathom the mystery and not dismiss it as a vagary. The author, in the course of an extended investigation into the effects of lightning, has come across many cases that have been called vagaries, but which on a close inspection have proved to be extraordinary only in the erroneous way in which they were described, and had they been correctly reported would have appeared perfectly consistent with pre-conceived ideas—in fact, could have been foretold in every case if the conditions that led to those effects had been known before the events occurred. The author reproduced experimentally several so-called vagaries of lightning, showing by means of rough models the conditions under which they occurred.

Miss C. O. Stevens also read a paper on "The Value of a projected Image of the Sun for Meteorological Study." She pointed out that by this method it has been ascertained that where the direction of movement of the atmosphere is tangential to the limb of the Sun the phenomenon of "boiling" displays a coursing or rippling character, and that where it is perpendicular to the limb of the Sun the character of the movements of distortion is that of springing in and out of the area of the Sun's image. Both these elements of movement are continuous even in the absence of all visible cloud, and it is possible not only to detect, but also to distinguish between overlying invisible atmospheric strata.

Chinese Records of Sun-spots and their Periodicity.

THE three principal periods which, according to my calculations, can be traced in the sun-spot activity may be derived, as I have already shown, from one cycle having 33·375 years as periodic time. Three successive cycles of this duration would fill one century almost exactly. We should, therefore, expect some recurrence of solar phenomena taking place in each century about the same year. I have recently examined the list published by

Mr. S. Hirayama (*Observatory*, vol. xii. p. 217, 1889), in order to see whether it contains evidence bearing on this point. Dividing the century into pentades, the following Table contains in the second column the total number of observations falling within that pentade summing up for all centuries:—

I.	II.	III.	I.	II.	III.
00–04	13	4	50–54	1	1
05–09	2	2	55–59	2	1
10–14	2	2	60–64	1	1
15–19	4	2	65–69	1	1
20–24	7	3	70–74	19	4
25–29	3	2	75–79	11	4
30–34	2	2	80–84	3	1
35–39	9	4	85–89	6	3
40–44	2	2	90–94	2	1
45–49	2	1	95–99	3	3

I think the meaning of the Table will easily be understood. Mr. Hirayama, *e. g.*, records observations in March, 826; on March 29, 1129; and on April 21, 1129; and there were no other centuries in which observations are recorded in the years 25 to 29 inclusive. Consequently the number 3 is entered in the second column of the Table as belonging to this pentade. Even taking account of the great uncertainty due to the incompleteness of the records, the result is somewhat striking. There are decided maxima in the three pentades 00–04; 35–39; 70–74. These divide the century into three nearly equal parts, and include the years (1837·2 and 1870·6) during which the most active maxima of last century were observed. As far as the Chinese records go, they form, therefore, a confirmation of a cycle of approximately 33⅓ years.

In order to eliminate the exaggerated effects produced by the obvious fact that an observer who has once had his attention attracted to an exceptionally large spot will probably follow the Sun carefully during subsequent years, and note spots which otherwise would have escaped observation, I have entered in the third column of the Table the figures obtained when all observations taken within a pentade in the same century are only counted as one observation. Thus, spots were observed in the pentade 70–74 during the fourth, ninth, tenth, and fourteenth centuries. The figure 4 is therefore entered in the third column as belonging to the interval 70–74. It is seen that this alteration in the method of reduction still leaves us with decided maxima of occurrence during the same pentades. The deficiency shown during the years 50 to 69 is also suggestive, in view of the almost complete absence of spots during these years in the seventeenth century.

We may reasonably take the entries from which Column III. is derived as independent of each other, for, assuming a succession of sun-spots not regulated by any periodicity, there seems no reason why the probability of an observation of a spot in any one pentade should be affected by the fact that a spot has been observed in the previous pentade. The increased attention of any particular observer which would decidedly affect the observations if a month or a year were taken as the standard time interval must at any rate have a much diminished influence after a lapse of five years, though it may not be possible to eliminate its effect altogether. Assuming, therefore, the distribution of sun-spots in successive pentades to be independent phenomena, we may calculate the probability that a periodicity of 33 years as marked as that shown in Column III. should be due to accident. I find that it is only one in 180,000, and some weight must therefore be attached to the evidence of these records.

This confirmation of the 33¼ year cycle brings into prominence its close agreement with the period of revolution of the Leonide meteors; it is to be noted that this swarm passes through perihelion at times which just precede the 33 years outbreak of sun-spots. The great meteoric displays of the Leonides in 1832 and 1866 took place during a time of minimum sun-spots, but the immediately succeeding maxima were, as already pointed out, the largest maxima of the century. One further coincidence, to which my attention was drawn by Prof. Turner, deserves to be mentioned. The first Chinese record of sun-spots is dated 188 A.D., while the Leonides were brought into the solar system in 126. The value to be attached to the fact that there is no record of sun-spots previous to the introduction of the Leonides might better be judged if we had more precise information as to the date at which Chinese annals of astronomical facts begin to be available. It is to be hoped that some one may be able to say whether Mr. Hirayama's list begins in the second century, because he has only examined the records as far back as this century, or whether there is really some ground for thinking that the Sun was free from spots previous to that date.

ARTHUR SCHUSTER.

A Note on the Figure of the Earth.

THE remark by a contributor in last month's number that the ellipsoid and spheroid * are getting out of date in geodesy, and that it is the fashion now to speak of the geoid, which is understood to mean the figure—whatever it may be—to which the plumb-line is everywhere normal, together with the recent publication from Greenwich of some longitude determinations, which may or may not prove to have some bearing on the question, has led to the compilation of this note collecting the evidence that has been adduced at various times on the subject of the shape of the Earth's equator—whether it be a circle or an ellipse, whether, that is, the Earth's figure is a spheroid or an ellipsoid.

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* The spheroid in geodesy is defined as the figure described by rotation of an ellipse which does not differ much from a circle about its minor axis. The ellipsoid is a figure bounded by ellipses of different ellipticity, but which have the same line for minor axis.

This problem of geodesy may be set down succinctly thus: if any meridian of the Earth be chosen, and the astronomical amplitudes of two arcs of this, that is the difference of latitude of the two ends of each, be determined, and the lengths of the two arcs be measured in feet, it is possible to determine the shape of this meridian from these data, the ellipticity of the curve, and the lengths of its major and minor axes in feet. If it were known with certainty that the shape of the Earth was a figure formed by rotation of an ellipse about an axis, the measurement of these two arcs of a meridian would theoretically give complete information about the shape of the Earth. Measurement of arcs of another meridian or of meridians would probably give other values for the lengths of the axes of those ellipses; and then arises the question, are these differences due to errors of observation, or are they real differences, and is the radius of the equator of the Earth not the same in all longitudes—that is, is the Earth an ellipsoid? A natural thought will at once occur. This problem might be better solved by measuring two arcs of the equator, or two arcs of a parallel, instead of arcs of meridians, and so find out the ellipticity, if any, of the equator or the parallel; but the objection felt to the solution has been that it involves determinations of differences of longitude, and these have hitherto been considered not to possess the necessary accuracy. This being the method of procedure, it will be well to consider how the observed facts have been actually treated.

Passing over early measures of arcs of meridian which had established the flattening of the poles of the Earth, and had determined with some accuracy its dimensions, this sketch may begin with Airy's determination, in 1830, of the figure of the Earth, which is believed by the writer to have been the first attempt to combine measures of several meridians to find a result. Airy had as his data the French arc of meridian from Formentera, an island near Minorca, to Dunkirk, the Indian arc as completed by Everest, the Russian arc measured by Struve, Lacaille's arc at the Cape of Good Hope, the English arc measured by the Ordnance Survey, the result of the classic measurements made by La Condamine in Peru, together with some other shorter arcs, and four longitude determinations or measures of arcs of parallel, which were considered of inferior value. The conclusion arrived at from discussion of these was that *on the whole* the Earth's surface could be represented by the figure described by the rotation about its minor axis of an ellipse with semi-axes 20,853,810 and 20,923,7 English feet respectively. The residuals were comparatively large, but that this was not due to any error or want of skill in treatment of the data is evident from the fact that some years later Bessel, apparently in ignorance of Airy's work, used practically the same data, and obtained essentially the same result.

In the year 1858, General T. F. de Schubert, of the St. Petersburg Academy of Sciences, essayed another discussion of the same, or rather nearly all of the same measurements. Finding discrepancies arising from the usual assumption that the Earth is a solid of revolution, and that all the meridians are equal ellipses, whose minor axis is in all cases the axis of rotation of the Earth, he was led to base his discussion on the hypothesis that the meridians are not equal ellipses, and arrived at the conclusion that the Earth's equator is an ellipse whose major axis lies in longitudes 41° 4′, 221° 4′ east of Greenwich*.

The publication of General de Schubert's determination, which met with considerable commendation, although suggestions for improvement of his methods were made, induced Col. (then Capt.) A. R. Clarke to repeat the discussion, omitting some of the less valuable data, and adding some determinations of latitude which were likely to improve the result. As to the method used it may be said that the formulæ of reduction were constructed on the assumption that the Earth is an ellipsoid with three unequal axes, and here it may be stated that results derived on this assumption will naturally give smaller residuals than will results derived from the assumption of only two unequal axes, since there are more unknowns which can be adapted to fit the observed facts and the principle of minimum residuals fails as a criterion. In this case Col. Clarke found the ellipsoidal figure of the Earth which best fitted the existing meridian measurements had for its equator an ellipse whose semi-axes differed in length by 5308 feet, or just about one mile, the major axis passing through longitude 13° 58′·5 east of Greenwich †.

In the last-mentioned memoir the author hinted at the value that measurement of arcs of longitude would have in the settlement of this question, and some years later these were at his command, when, in 1878, eleven telegraphic determinations of longitude in India with their corresponding geodetic amplitudes were published by the Indian Survey. Col. Clarke again made a determination of the elements of the Earth from the meridian arcs, Indian, Anglo-French, and Russian, now improved since his last investigation; but he found that the spheroid of revolution deduced from these did not not fit the measurements of the surface of India, but that the Indian longitudes better fitted an ellipsoid, the semi-axes of the equator differing by about 1500 feet, the longer axis passing through longitude 8° 15′ west of Greenwich ‡.

We are fully aware of the large effects on geodetic measurements that have been said to be due to the peculiar configuration of the

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* See Table at end.

† Memoirs R.A.S. vol. xxix.

‡ 'Philosophical Magazine,' August 1878.

Indian peninsula—to the fact that its southern portion is surrounded by the ocean, and that its northern territories consist of high lands bounded by the immense Himalayas. No doubt there is ample evidence for the existence of Local Attraction—that is, that the plumb-line in many places does not hang normal to either a spheroid or an ellipsoid; but it seems rather to be evading issues and neglecting facts of observation if all discordances are to be ascribed to Local Attraction, as is implied in the following sentence by Archdeacon Pratt: "Local Attraction appears to supply a source of correction which makes a resort to so peculiar an hypothesis as an ellipsoidal mean figure unnecessary and untenable"; but the Archdeacon also says: "If a very large number of arcs in all parts of the world were measured and, local attraction being taken into account, the result gave an ellipsoid with its two equatorial axes differing by a quantity important when compared with the residual errors of observation there might be some argument for an ellipsoidal figure." Though there are not yet a very large number of arcs available, there are certainly two longitude arcs which go to support the result derived from those of India—namely, that the Earth's equator is an ellipse whose semi-axes differ by about one or two thousand feet.

The Geodetic Survey of South Africa now in progress under the direction of Sir David Gill supplies some information which seems to be well suited for elucidation of this question because of its situation 60° west of India—if it were 90° west it would be better. In the course of this survey two longitude arcs have been measured, from the Royal Observatory to Port Elizabeth, and from Kimberley to Durban, astronomically and geodetically, each extending over about 7° of longitude, and in each case the astronomical arc is greater than the geodetic, computed on the assumption that the equator is a circle whose radius is 20,926,202 feet*, by about [illegible]″. Sir David Gill shows that this difference would be reduced to about 1″ if Airy's elements were used—that is to say, the measurements of the Cape Survey imply that the curvature of the Earth in that region is more than that of a spheroid with the above equatorial radius.

The longitude determination published from Greenwich, mentioned above as being possible evidence on the question, is the arc from Greenwich to the west of Ireland, the complete history of which begins more than sixty years ago. About that time it was proposed to measure a long arc across Europe on the parallel of latitude 52°, from the east of Russia to the Atlantic. Sir George Airy promptly did his part by measuring the difference of longitude between Greenwich and Valentia, an island off the coast of Kerry—once, in the year 1844, by transmission of chronometers, and again telegraphically in 1862, when that method was invented. Different stations in the island were used on the two occasions, and both times the astronomically determined value of the arc was larger than the values found by the measurements of the Ordnance Survey *. In the first case, when the longitude of Feaghmain was measured, the astronomical result was larger by $0^{s}{\cdot}59$; in 1862, when Knightstown was the western station, the astronomical arc was in excess by even more, $0^{s}{\cdot}72$. As some evidence that these excesses were not due entirely to errors of the astronomical work, it may be said that the longitude of a third station, Foilhomurrum, in Valentia, was incidentally determined by American astronomers when measuring a trans-Atlantic arc, and this result was larger than that of the Ordnance Survey by $0^{s}{\cdot}58$.

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* Clarke's last value: 'Geodesy,' 1880.

These differences, which, it will be seen, would not exist if smaller values of the elements were used, were ascribed to local attraction. It was said that since Valentia was an island and was in the neighbourhood of mountainous country, the plumb-line did not hang normal to the mean spheroid, and that the level-bubbles did not take the form of the mean surface of the Earth at this place, but the measured differences were larger than could be accounted for by computation of the attraction on any ordinary hypothesis.

This explanation was therefore not considered final, and in 1892, when opportunity offered, the present Astronomer Royal arranged that the longitude of Waterville, on the mainland of the west coast of Kerry, should be determined. This was done, and again the astronomical arc was larger than the geodetic by $0^{s}{\cdot}37$. Waterville is west and south of the Macgillicuddy Reeks, the mountainous district of Kerry, which might be thought to be the cause of some local attraction, and it was part of the Astronomer Royal's plan to measure the longitude of another station where this attraction might act in the opposite direction, so that its effect might be eliminated from the mean.

In 1898 the longitude of Killorglin, a town to the north-east of the mountains, was measured, and the astronomical determination again came out the larger, this time by $0^{s}{\cdot}15$. These seem to supply some evidence of local attraction, but it is difficult to ascribe to each determination the true correction for this effect, and no attempt can be made to do this here; but by simply taking the mean of the results of Waterville and Killorglin, which are fairly symmetrically placed with respect to the mountains, we are left with the fact that the astronomical arc is greater than the geodetic by a quarter of a second of time, or 4″, in an arc of about 10° of longitude. This difference is very similar to that found at the Cape, and would not exist if the elements of the Earth used in the geodetic computation had been diminished by a nine-thousandth part—that is, if the equatorial radius of the Earth had been diminished by about 2300 feet, a quantity of the order found by other means to be the difference between the major and minor axes of

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* The elements of the Earth used in the Survey computations are Clarke's first values $a=b=20{,}926{,}348$ feet, $c=20{,}855{,}233$ feet, which do not differ much from Clarke's values used in the Cape geodesy.

the hypothetical ellipse. So that, while it can scarcely be thought feasible to determine the dimensions of the Earth, or the exact form of its equator, from the measurements of arcs so small as ten degrees, it seems worth pointing out that the longitude determinations of India show a less curvature, whilst those of the Cape and the Anglo-Irish arcs show a greater curvature than that of the mean spheroid, and that these differences of curvature are in their quantity and direction consistent with the elliptic figure of the Earth's equator, which had previously been determined from measurements of arcs of meridian.

	Lengths of semi-axes in English feet.			Longitude of major axis.
	a.	*b.*	c.	
Airy	20,923,713		20,853,810	
de Schubert ...	20,927,242	20,924,888	20,855,605	41° 4' E.
Clarke (1860) ...	20,926,485	20,921,177	20,853,768	13 58 E.
Clarke (1878) ...	20,926,629	20,925,105	20,854,477	8 15 W.

H. P. Hollis.

With the British Association in South Africa.

In the last batch of these notes we passed from Cape Town to Johannesburg in the course of a sentence, and said nothing of the nine days' journey which intervened between the two parts of the formal meeting. We must go back to the last Saturday at Cape Town, and board the 'Durham Castle' as she gets under way for Natal amid the loud farewells of our good hosts. Some of the party have gone ahead on the fast 'Saxon' to call at ports upon the way, and it is the business of the usually slow 'Durham Castle' to show that she can do sixteen knots and get to Durban first, a feat which was accomplished to the no small discontent of those who had spent twenty-three days on her coming out at eleven knots all the way from England.

South Africa is a great place for papers of the semi-serious kind. The Cape Town *Owl* with its weekly award of a cake was an old friend, for had we not all heard of how in 1901 it awarded the cake to His Majesty's Astronomer because he did not discover the comet? The Bloemfontein *Critic* came out with a cartoon of the "British Association Cake-Walk," headed by a bald-headed person with flowing locks and moustache, with a telescope under one arm, and a lecture ("The Milky Way") under the other. At Durban we had the representative of the Harbour Engineer explaining his works. "You pour gold into it," he explained. "Well, what happens then?" "Well, then . . . . then you pour more gold into it." At last the result is successful, and where only a short while ago the passenger by mail steamer was swung on to the tender in a basket, the biggest vessels now steam straight in, a tug butts them round the corner by the nose, and in five minutes they are berthed in the beautiful lagoon.

In a short while we found our way through the famous Botanic Gardens to the Observatory, where Mr. Neville plays the arduous parts of Government Astronomer, meteorologist, analyst, and lunar theorist, with the assistance of two or three old acquaintances from Greenwich. Two features of the Observatory remain in the memory—the excellence of the arrangements for the time-service, and the way in which white ants, in spite of all discouragements put in their way, persist in enjoying the Greenwich observations in preference to all other reading matter.

The recollection of our days in Natal is a medley of memories of the kindest hospitality and the most incongruous experiences. To be served at dinner by an Indian coolie and a Zulu boy who also acts as nursemaid; to come tearing down from the Berea to the station in a ricksha drawn by a leaping Zulu decked with cows' horns and monkeys' tails; to climb mountains in a dining car on the way to "Sleepy Hollow," as the Durbanites call Maritzburg, and there attend a lecture on sleeping sickness, a complaint most contagious after a day in the highlands of the native reserve, assisting at the wedding-dance of a chief; to remember the Governor seated at the function as supreme chief of the tribe; the President, our Plumian Professor, courageously receiving Kaffir beer at the hands of beautiful maidens in beads; and the bride, as we last saw her, with a long knife in her hand, heading the chase of the cattle that were to be killed for supper —these, with a thousand other memories belong to three days, and then we were suddenly transported north to the Mooi River and Frere, where the white graves by the line marked the entrance to the scene of the war.

The battlefields in Natal are over much bemonumented. It is excellent to mark the place where the Devons charged and cleared the top of Waggon Hill on the day when Ladysmith was hard pressed, but why erect a larger pile to show where the guns were lost at Colenso? How they were lost is easy enough to see, for one can follow the watercourse up from the bed of the Tugela to the spot where a few daring marksmen enfiladed the battery at a range of 100 yards; though why no one looked into that ditch is another matter; the same, perhaps, as will explain why it was supposed that the easy walk to the top of Hlangwane Hill was barred by the river. In fact, the field of Colenso is a depressing place in which to spend the night, even in a comfortable train with the first mail from home just delivered; and it was pleasant to start next morning for Ladysmith with its more satisfactory recollections.

At Ladysmith they show you the place where the "Chevril" was made, and the tower of the Town Hall with one corner gone; and they press you to carry away live pom-pom shells as mementos. Otherwise there is little to show that the pleasant town was so recently besieged, until you go up to Waggon Hill and Cæsar's Camp, and see the sangars and the graves—surprisingly few sangars there are, too, for the hills are flat-topped like all South African hills, and apparently the defenders kept down below the inner edge and swept the top with their fire.

The ascent over Laing's Nek is like a series of trips from London Bridge to Charing Cross *viâ* Cannon Street, in one respect, that the train is reversed back for front every few minutes. When the gradients are too steep to be negotiated by continuous curves, they lay out the line in a series of cusps; you burrow into the mountains a little way; then the engine comes round and pulls you out up the other branch. Experienced travellers had warned us of the horrors of railway travelling in South Africa, and we found it delightful. You never go fast enough to get tired; you can always get out at the crossing-places and collect flowers and water-melons and scorpions, and gradually you get expert in judging how long a start you can give the train and catch it. But beware of the "wait-a-bit" thorn, especially if you are a stout German astronomer with a melon in each hand and a sun-helmet that falls over your eyes.

The astronomer who does not care for pumpkins has one great advantage on these trains, that there is nothing else for him to collect, and his pursuits are inoffensive. Very different is it with the entomologist. In one compartment of train A there lived a naval captain, a physicist, and an entomologist. The third was always bringing scorpions back with him. The second, in self-defence, had to collect scorpions too, and being a man of resource he soon improved upon the methods of the third. At one stop he found on the track a piece of hoop-iron; with the rail as an anvil in five minutes a pair of scorpion tongs was forged, and he could handle with impunity bigger beasts than the entomologist cared to tackle. But what the first man, the naval captain, thought of his quarters and messmates had better not be recorded.

The approach to Johannesburg from the east is made impressive by the mountains of tailings all along the Rand—mountains that are in many cases themselves gold mines, for the amalgamation process catches only about 65 per cent. of the gold, and before the cyanide process was introduced the rest went into the tailings heap. Nowadays, when they get 98 per cent. out of very poor ore treated at enormous speed in huge quantities, the difference between profit and loss appears to lie, as usual, in the treatment of what used to be waste. The most impressive feature of both gold and diamond industries is the rate at which everything is worked. At one of the Kimberley mines a load of blue ground is delivered to the washing-plant every five seconds, and that goes on day and night, with the result that the De Beers Company were able to show us diamonds in heaps, worth a quarter of a million, and yet complained that they could not produce within a million carats a year what they would like, so great is the demand from America.

At Cape Town in an evening lecture we had heard how everything, from the prevention of malaria by oiling the mosquito breeding-pools right away to the recovery of diamonds and the latest thing in wireless telegraphy, depends upon surface-tension. At Kimberley we saw the diamond process working, and it is quite the most fascinating thing to watch that can be imagined. By some peculiar surface-tension effect diamonds and one or two other gems will stick to wet grease, and inferior stones will not. So all the collection of small gravel that is washed out of the blue ground is passed in a thin sheet of water over joggling corrugated plates smeared with grease. The diamonds stick without fail; not one is lost; but all the waste stones pass on and become the "pulsator gravel" of which all the golf greens and tennis and croquet lawns of the country are made. The researches of a distinguished astronomical traveller have made us familiar with the golf links of the West. One of the many things that made us regret his absence from the South African party was the fact that he missed the experience of putting on a green of garnets—for small garnets occur in tons in the diamond ground and are thrown away with the gravel as of no account. The nett result of the "greaser" process is the funniest mixture in the world—diamonds and all the odd bits of scrap metal from blasting cartridges and boots, for metals stick to the grease. So the men in the diamond office sit sorting out their gains into two buckets—diamonds in one, old nails in the other. . . . .

The great event at Kimberley was Sir William Crookes' lecture, illustrated with the most splendid experiments. The exhibition of diamonds he had made left the chairman of De Beers unmoved, for the results were both microscopic and expensive. But it was comic to see the rage of the diamond dealers when he exhibited a diamond that had been turned a beautiful green colour by the action of radium. If this kind of thing were allowed to go on, what would become of the trade in fancy stones? The excitement at the Club that night was in inverse proportion to its knowledge of radio-activity, but there was a unanimous opinion against the making of ordinary diamonds into fancy stones, either by science or devilry.

The only point upon which Kimberley was equally unanimous was that the Premier Diamond Mine near Pretoria was of no account. It might have produced a diamond weighing three thousand carats—in fact it had, for we shook hands with the man who found it, and saw the spot where he noticed a flash in the bank, began to dig with his penknife, and found that he could not get round it till he had made a hole bigger than his fist—but what are mere surface workings compared with the Kimberley pipes? However, our good friend the founder of the Premier Mine seems to have been inspired when he bought the land from an old Boer without being allowed to examine it or even go on it, for *his* pipe is really about half a mile across, and its hard polished reddish walls filled with the blue ground are a wonderful sight.

A. R. H.

Recent Changes on Jupiter.

During the nearly past opposition of Jupiter, the N. equatorial and N. temperate belts have been extremely faint. But there has lately occurred a remarkable outbreak of dark material from the N. tropical zone. This material apparently flows obliquely in a W.S.W. direction, effecting a junction with the N. equatorial belt and rapidly extending itself along that belt, which promises to shortly become quite a conspicuous object throughout its circumference.

At the *f.* end of the new feature there is a large dark spot (longitude 77°), representing the seat of activity and moving at about the same rate as the great red spot.

I first detected the recent disturbance while observing Jupiter in sunshine on April 10, with a 10-inch reflector, power 220. I had noticed nothing unusual in the same region of the planet on March 24, April 3 and 7, but observations on those nights were rather hurried and my attention was chiefly engaged with the red spot and its, at present, interesting and variable surroundings.

The new spot in the N. tropical zone should be looked for near C.M. at following times:—

	h	m		h	m
May 4	7	2	May 11	7	52
6	8	41	14	5	23
7	4	33	16	7	2
9	6	13	21	6	13

Observations of the preceding side of the darker section of the N. equatorial belt should also be made. The markings can be very distinctly seen in sunshine.

Bishopston, Bristol,
1906, April 25.

W. F. Denning.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Sosigenes.

Gentlemen,—

Not much is known of this Greek astronomer, but his name is found in all books on the science which treat of the calendar, and in all Roman histories, on account of the assistance which he rendered to Julius Cæsar in his much-needed reform of their calendar. For this reason he has been placed next to Cæsar in the Moon, between Mare Tranquillitatis and Mare Vaporum, Julius Cæsar being a large irregular formation and Sosigenes a small circular ring-plain 14 miles in diameter, to the west of it, in latitude about 11° north and longitude 18° west.

Most books speak of Sosigenes as an Alexandrian, but there is no proof of this, and M. Tannery well remarks, in 'La Grande Encyclopédie,' that "rien ne prouve que Sosigène ait été originaire d'Egypte, ni même qu'il ait appartenu à l'école d'Alexandrie." Until the mistake was pointed out by Theodore H. Martin, little more than a quarter of a century ago, he was confounded with another Sosigenes, a peripatetic philosopher who lived about a century later and whose pupil, Alexander of Aphrodisias in Cilicia, was the author of a well-known commentary on Aristotle. (There was also a stoic philosopher named Sosigenes in the second century after Christ.)

All we know of Cæsar's Sosigenes is derived from Pliny, who tells us (xviii. 57) that he had a difficulty in making up his mind about the true length of the year, and composed three treatises upon it. His data for the heliacal risings and settings of the stars at different seasons were doubtless used by Virgil in the 'Georgics.' Cicero's joke, as related by Plutarch, is well known. When some one said to him "Lyra will rise to-morrow," he replied: "Undoubtedly, for so it has been commanded"—*ναὶ, ἐκ διατάγματος* (Vit. Cæs. 59). Merivale thinks that the joke was spoiled by the mistake of the reporter. What was meant was that, by the new arrangement, the autumn season commenced (halfway between the summer solstice and the autumnal equinox) at the time of the setting of Fidicula, as the Romans usually called Lyra. With us, of course, that constellation never quite sets, but it does for a short time in Italy.

Pliny also tells us (ii. 8) that Sosigenes agreed with Timæus in assigning twenty-three degrees to the greatest angular distance of the planet Mercury from the Sun. It may really exceed this by nearly five degrees, but of course the point is difficult to determine with accuracy on account of the great eccentricity of Mercury's orbit. Timæus was nearly right, according to Pliny, with regard to the greatest elongation of Venus; he does not report any determination of this by Sosigenes. Yours faithfully,

Blackheath, 1906, April 2. W. T. LYNN.

The Period of β Lyræ.

GENTLEMEN,—

The timely article in the February number of the *Observatory*, in which Mr. Roberts suggests the application of Prof. Darwin's tidal theory to explain the increasing period of β Lyræ, leads me to point out still another possible cause for producing a change in the periodic time of revolution.

Solar observations and certain studies relating to the formation of spiral nebulæ indicate that an incandescent body has the power to remove a part of its mass, in a finely divided state, to great distances, even beyond the sphere of its own sensible attraction. At a certain stage in the life-history of such a body the decrease in mass may be so rapid as to produce a sensible increase in the periodic time of any body belonging to the system. Obvious changes will result if a part of the ejected mass later on returns to the parent body.

Very sincerely yours,

Ann Arbor, 1906, Mar. 21. J. M. SCHAEBERLE.

Shadow-bands.

GENTLEMEN,—

With reference to the note on Shadow-bands in the April number of the *Observatory*, it may be of interest to record an observation similar to that made by M. Dufour in 1852. Some years ago when at Cambridge I had rooms on the King's Parade; I used frequently to notice that when the Sun set over the roof of King's College Chapel irregular bands of shadow used to move vertically upwards on the sunlit walls of the room. As far as I recollect, this phenomenon used to continue for more than one or two seconds; if I remember rightly, the bands used to make their appearance when about half the Sun was hidden behind the roof and used to continue as long as any sunlight fell on the wall of the room.

Yours faithfully,

Ditcham Park, Petersfield.
1906, April 19. CHARLES J. P. CAVE.

OBSERVATORIES.

U.S. NAVAL, WASHINGTON.—After a somewhat exciting period of re-organization and re-staffing, which resulted in Rear-Admiral C. M. Chester being placed in command, this establishment appears to have settled down to a condition of peaceful routine, as will be seen from the following summary of work with the principal instruments made from the Report of the Superintendent for the year ending 1905, June 30.

With the 9-inch transit-circle 9179 observations were made, chiefly of stars in Newcomb's List of Fundamental Stars, and of the stars in Sir David Gill's Catalogue of Zodiacal Stars, but the reductions of the observations made with this instrument are much in arrear owing to lack of computers. The 6-inch transit-circle was not used during the year. A self-registering micrometer of the Repsold type is being made by Messrs. Warner & Swasey for use with this instrument. This department is in the charge of Prof. Eichelberger.

The new Altazimuth is in the charge of Prof. F. B. Littell. The number of observations secured during the year was 1896; with the Prime Vertical Instrument, under the charge of Mr. G. A. Hill, 625 observations were made. It appears that this instrument is used for observations of the Moon, and it is expected that a useful series of results extending over a period of one revolution of the Moon's node will soon be published. The equatorials were used for observations of comets, satellites, and asteroids, under the management of Prof. Skinner, who is also devoting much time to the completion of the zone of the Astronomical Gesellschaft Catalogue −14° to −18°.

The establishment of a branch observatory at Tutuila, Samoa, proposed by Admiral Chester a year or two ago [see vol. xxvii. p. 135], is now practically an accomplished fact. A transit-circle house and a clock house at constant temperature have been built, and at the date of the report it was expected that the instruments would soon be erected, and work, which is chiefly to observe stars from the South Pole to beyond the zenith of Washington, would be begun.

The preparation of the American 'Nautical Almanac' is still in the hands of Prof. Harshman, U.S. Navy.

PUBLICATIONS.

Our Own and Other Worlds.—We have lately had brought to our notice a book with this title and an Introduction which together would condemn any book. The reverend gentleman who writes the latter says that the author "makes the conception of the vast spaces of the Universe more real by using the journey of a railway train," and, again, "the argument in favour of other worlds being inhabited is exceedingly strong and cumulative," which two statements were amply sufficient to indicate to the accustomed reader of astronomical literature the style of the book. Experience at once marked it as of the indulgently patronizing and possibly inaccurate class, and in neither particular was the prediction falsified. But this author puts himself on too high a pedestal. He quarrels with Laplace for his nebular hypothesis, with Sir Robert Ball for his theory (does Sir Robert claim this?) of dark stars, with Dr. Wallace for his suggested arrangement of the Universe, but lays down the law himself on several points in no uncertain manner. His arguments for the fact of other planets being inhabited are the well-known one of analogy, and the fact of the waste of creative power in the Universe if it were not so; and to these one can have no objection; but besides some contradictions—at one place it is definitely stated that Mercury is incandescent fire and Neptune is a zone of frost, in others that the first might be a quite comfortable world and that Neptune need not be cold—and inaccuracies, the style of the book is not pleasing, and it is doubtful whether a work intended for elementary education should have been written in so controversial and in some cases flippant a vein. The following extract illustrates the style of the book:—

> Astronomers admit that planets are more or less deflected from their path; but the astounding thing is that these deflections are rectified. How they are

rectified I believe no man can tell. I have read an elaborate account of how they are rectified, but it only left me in a haze. I think it would be as well to say "I don't know."

Why did not this modesty prevail throughout?

NOTES.

Comet Notes.—MM. E. Schoenberg and E. Büss have deduced improved elements of Comet 1905 *c* (Giacobini). The observations used extend from 1905 Dec. 7 to 1906 Jan. 14:—

T	1906 Jan. 22·3927 Berlin M.T.	
ω	199° 11′ 40″	1906·0.
☊	92 5 6	
i	43 39 39	
log *q*	9·33427	

Herr Strömgren has deduced new elements of Comet 1906 *c* (Ross) from observations made on March 19, 22, 29:—

T	1906 Feb. 20·756 Berlin M.T.	
ω	274° 46′·4	1906·0.
☊	71 47·7	
i	84 36·8	
log *q*	9·8492	

Ephemeris for Berlin Midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
May 4	3 50 32	25 46	May 12	4 3 57	28 54
8	3 57 19	27 22	16	4 10 27	30 21

The brightness is rapidly diminishing, and the comet will scarcely be visible so near the Sun.

The following elements of Comet *b* 1906 (Kopff) are by Messrs. Crawford and Champreux from observations made at the Lick Observatory on March 5, 15, 27:—

T	1905 Oct. 20·802 G.M.T.	
ω	159° 3′·1	1906·0.
☊	342 9·9	
i	4 12·6	
q	3·3165	

Ephemeris for Greenwich Midnight.

Date.	R.A. h m s	N. Dec. ° ′	Brightness.
May 8	11 21 12	2 12	0·58
16	11 23 8	1 57	
24	11 25 55	1 36	0·45

A. C. D. C.

MINOR PLANET NOTES.—The following planets have been discovered:—

Letter.	Date.	Discoverer.	Magnitude.
TP	1906 Mar. 14	Kopff.	12·3
TQ	17	Wolf.	13·2
TR	17	"	13·1
TS	18	"	13·0
TT	20	Kopff.	12·0
TU	20	Wolf.	13·0
TV	21	"	13·2
TW......	27	"	13·0
TX	27	"	13·3
TY	27	"	13·0
TZ	27	Kopff.	11·6
UA......	Feb. 21	Wolf.	11·6

Another planet OO^a has been found on two plates of the Andromeda nebula taken by P. Götz, 1904, Aug. 12.

Planets 543, 546 have been named Charlotte, Herodias.

TC, discovered on Feb. 21, has since been found on a plate of Jan. 22.

UA was at first supposed to be identical with 546, but this was subsequently found not to be the case.

Dr. Berberich has deduced circular elements of TG, which give a mean distance of 5·05, almost the same as Jupiter's; it is of course probable that the orbit is elliptical, and the planet near aphelion, but in any case it is an interesting object.

TW is interesting from its considerable distance from the ecliptic (30°) and its great motion in declination (27′ N. daily). It has probably a small mean distance and a large inclination.

A. C. D. C.

VESUVIUS AND SAN FRANCISCO.—The two great natural catastrophes which have happened during the past month, the eruption of Vesuvius between April 5 and 17, having a maximum on the 7th and 8th, and a terrible earthquake at San Francisco on April 18, which practically destroyed the city, call for scarcely more than mention here, as they have little astronomical bearing, but the following extract from an article in *Nature* by Prof. Davison may be found of interest. It may be added that he gives the time of the earthquake as $1^h\ 13^m$ P.M. Greenwich Mean Time, or perhaps a few minutes earlier.

> In all parts of the world delicate seismographs soon afterwards recorded the occurrence of a violent earthquake. The first waves reached Victoria (B.C.) at 1.16 P.M.; at Washington the movement was so strong that the pen passed off

the recording sheet. In a quarter of an hour the seismographs of Great Britain took up the tale, large disturbances being recorded at Shide, Bidston, and Edinburgh; at Birmingham the pointer of the Omori horizontal pendulum swept three times off the drum. Passing over to the Continent, they set to work the instruments at Berlin, Heidelberg, Vienna, Laibach, Turin, Rome, and many other places. The pendulums at Florence shared the fate of those at Washington and Birmingham. The seismograph at Cape Town also registered the movement, while those in Japan were disturbed by the waves proceeding in the opposite direction across the Pacific. Only the scantiest details are as yet made known, but, if we may judge from the diagram at Birmingham, the complete series of records will be one of great interest and value.

The first series of preliminary tremors reached Birmingham at $1^{h}\ 25^{m}\ 3^{s}$ P.M. (G.M.T.); they were small in amplitude and had an average period of 6·4 seconds. At $1^{h}\ 35^{m}\ 7^{s}$ they were followed by the second series of preliminary tremors, much larger in amplitude and with an average period of 11·4 seconds. These tremors, as is now well known, traverse the body of the Earth with velocities of about 10 or 11 and 5 km. per second respectively. At $1^{h}\ 45^{m}\ 13^{s}$ began the principal portion of the movement, consisting of undulations which travel over the surface with a nearly uniform velocity of 3·3 or 3·4 km. per second. In the initial phase of this portion the undulations had an average period of 44·1 seconds; in the slow-period phase (which began at $1^{h}\ 50^{m}\ 22^{s}$) of 25·2 seconds, and in the succeeding quick-period phase of 16·2 seconds. Unfortunately, this portion of the record is incomplete, for the pointer of the pendulum swept off and on the drum three times, several waves being thus lost, and the initial epoch of the quick-period phase cannot be determined. The end-portion of the disturbance began at $2^{h}\ 1^{m}\ 4^{s}$, and consisted of a long series of unusually clear and regular waves with an average period of 15·0 seconds. The duration of this portion is uncertain, for these waves were reinforced at $3^{h}\ 28^{m}\ 38^{s}$ by the undulations of the principal portion which travelled through the antipodes along the major arc joining San Francisco at Birmingham. At $3^{h}\ 56^{m}\ 57^{s}$, however, the trace becomes nearly steady, but a careful examination reveals another series of long, low undulations from $4^{h}\ 58^{m}\ 32^{s}$ to $5^{h}\ 6^{m}\ 34^{s}$, which represent the return of the first series of surface-undulations after they had completed the tour of the globe and travelled once more as far as Birmingham. The interval between the first and third passages of these waves is $3^{h}\ 13^{m}\ 19^{s}$, and corresponds to a mean velocity of 3·36 km. per second.

THE LIVERPOOL ASTRONOMICAL SOCIETY.—This local Society begins a new series of its 'Transactions' with the publication of a series of original observations, those of the eclipse of August last. The Liverpool Society tried to arrange an expedition on rather a large scale, but this did not arrive, and five Members only observed the total eclipse, four at Burgos and one from the deck of the s.s. 'Arcadia.' The observations of these gentlemen are now before us. Mr. Killip and Mr. Benson had rather extraordinary good fortune at Burgos to have a clear break at time of totality in an otherwise cloudy sky, and made good use of it to take smoe excellent photographs of the corona, which were shown at the November Meeting of the R. A. S., and copies of which are published in this pamphlet. It is worthy of note that these photographs, one of which was exposed for 3½ minutes, were taken with cameras on what might be considered inadequate mountings and without driving-clocks. Other reports are by Mr. Thomas Taylor, whose plan of action was to note what naked-eye objects could be seen, and saw Venus easily before totality and also Regulus, but did not see Mercury, which was possibly hidden by cloud; and by Mr. E. Dickson, who obtained a very successful photograph of the flash-spectrum with a Thorp grating on a Schleussner's plate. The observer on the 'Arcadia' was Mr. Longbottom, who frankly states that he went for the trip and not to make scientific observations, but, with Mr. S. A. Saunder, made some careful notes about shadow-bands and saw the corona very nicely.

THE FIGURE OF THE SUN.—It may be remembered that there was a note in the number of November last, on a discussion of measures of photographs of the Sun made by Dr. G. L. Poor, which, he concluded, showed that the shape of the Sun altered in a period coincident with the sun-spot period. Dr. Poor has since published another paper to the same end, using measures made by Dr. Ambronn and Dr. Schur made with the Göttingen heliometer extending from 1890 to 1902. These measures were originally made to detect any possible change in the mean diameter of the Sun, and the result of their discussion showed no such change. Dr. Poor has used the data in another way, and deduces a table as under, which shows the mean excess of the Sun's polar diameter over the equatorial as found by Schur and Ambronn respectively in each year:—

	Schur.	Ambronn.		Schur.	Ambronn.
	"	"		"	"
1890 ..	+0·13	+0·12	1896 ..	−0·05	−0·01
1891 ..	+0·06	+0·14	1897 ..	+0·02	−0·04
1892 ..	−0·06	+0·07	1898 ..	+0·11	+0·07
1893 ..	−0·09	−0·01	1899 ..	+0·05	−0·03
1894 ..	+0·10	−0·07	1900 ..	+0·02	+0·02
1895 ..	+0·04	+0·03	1901 ..	+0·43	+0·06

To our mind these figures do not seem sufficient evidence on which to base a conclusion so striking, but Dr. Poor thinks that they tend to confirm the result given in his former paper.

THE DROUGHT OF 1906, MARCH–APRIL.—An absolute drought is defined as a period of at least 14 consecutive days without rain, but the interpretation of this is naturally modified by the "accuracy" of the rain-gauge. The ordinary official readings are taken to hundredths of an inch, so that falls of less than ·005 inch (the adopted limit of a "rainy day") are ignored. At Greenwich, however, commencing with 1877, readings are taken to the third decimal place, so that even so little as $\frac{1}{1000}$ of an inch may be taken to break an "absolute" drought.

The statistics of recent droughts will show the difference clearly, such droughts occurring on an average about once a year. There is, however, apparently a greater probability of drought occurring in March–April than the yearly average would indicate.

In the period since 1877, January 1, the figures for this time of year are as follows:—

	(1) Limit ·005 inch.	(2) "Greenwich" limit.
1881....	March 26–April 11.	
1882....	March 27–April 12.	March 27–April 12.
1883....	March 31–April 17.	March 31–April 13.
1892....	March 29–April 12.	March 29–April 12.
1893....	March 18–April 16.	March 24–April 16.
1894....	March 16–April 1.	
1906....	March 27–April 13.	March 27–April 13.

The coincidence of dates is very remarkable, as also is the grouping of the years, especially with the higher limit, though the obvious 11-year period, as usual, breaks down after a temporary success.

The Greenwich sunshine records, with slight discontinuities owing to change of instrument, go back to the same date, 1877. By far the sunniest April of the period has hitherto been 18[illegible]3, which will be noted as having produced the longest spring "drought." But until the very last days of this month it was uncertain whether 1893 would not have to yield to 1906. The question is now decided against the current year, whose total, owing to the break in the latter half of the month, is only second of the thirty April registers.

Attention may be called to the close conjunction of Mars and Venus on Sunday, May 6. At two in the afternoon the planets will be separated by only 5′, Venus being south. Venus will be in conjunction with Jupiter on the morning of the 12th, separated by 71′, and Mars with Jupiter on the 18th. The planets set soon after the Sun, but they will form interesting configurations during the first half of May.

Owing to the fact that the first Sunday in June this year will be Whit-Sunday, the Greenwich Visitation will take place on Wednesday, May 30.

The next Meeting of the Royal Astronomical Society will be on Friday, May 11. As the Greenwich Visitation will happen on the last Wednesday in May, the date of the Meeting of the British Astronomical Association has been altered to May 23.

From an Oxford Note-Book.

With the advent of the present Government, the arena of conflict in matters relating to South Africa seems to have become established in the Houses of Parliament, so that many people may hear with some surprise that a series of important cricket matches has been played in the Colonies themselves, with very poor success on the part of Mr. Warner's M.C.C. team. The fact has not, however, escaped the notice of our esteemed and newly edited contemporary *Punch*; and in the issue for March 28 Mr. Warner is rallied upon his poor success in a series of burlesque forecasts of future tours and his accounts of them. He is imagined to visit Greenland and "lose the blubber"; to journey to China and fall before the great BO-LING and KA-CHING; and the name for his book on this enterprise is to be *Mandarin and Warnerout.* But it is the final forecast which gives an excuse for this reference:—

> From SIMS's hairless telegram dated Wellsville, Mars, June 29, 1909:—The third and last of the test matches between the M.C.C. and the All Mars Eleven was played yesterday on the Campus Martius, the beautiful ground of the metropolis of the red planet. Mr. WARNER, who won the toss, was unfortunate in being bowled by the last ball of the first over, the popular skipper having never become wholly acclimatized to the planetary atmosphere, but Captain WYNYARD created great enthusiasm by hitting a longhop into one of Schiaparelli's Canals, for which no fewer than eight were run before the ball was recovered. The innings closed for 42, of which FANE was responsible for a freely hit 33, to which the Marsupials responded with a useful score of 469. M.C.C.'s second innings yielded 70, to which Mr. WARNER contributed the second figure, and thus the game and rubber were won by the home team. Mr. WARNER's record of the tour, which he hopes to complete on the home journey, will, we understand, bear the attractive title of *Plum in the Planets.*

THE following is the opening sentence of an article on "Woman Suffrage" in the *Independent Review* for April:—

> Mr. Goldwin Smith, the most reasoning of all the adversaries of the political action of women, thinks, as Galileo's adversaries did of the Sun, that there is no intrinsic motion in Woman Suffrage.

Query, What *did* Galileo's adversaries think of the Sun?

THE February number of the *Astrophysical Journal* concludes with reviews of no fewer than 8 books, devoting rather more than a page to each. May one, without risk of being misunderstood, welcome the reviews even more cordially than the books? The pace has been recently so hot in astronomical research that there has been little enough time to write books, but less still to read them. And, at the risk of invidious comparison, we wonder who the reviewer "J" is. The proper place to look for him is in the list of Associates of the Royal Astronomical Society; but it contains only one name under J, that of Jules Janssen, and the review in question is not in the well-known style of that author. But it has an excellent style of its own. "How are ordinary meridian altitudes observed," it asks the author of the work under review, "when the Sun's declination at noon is almost equal to the latitude, when for a number of minutes no one can tell within a quadrant or two which point of the horizon is under the Sun?" The reviewer has, we may adventure to state, been at sea and in the tropics. "The book contains many such things." (Doesn't he mean the review?) "On page 241 there is a sextant example of which the venerable observations were made in 1808"; and the review concludes by "recommending the volume to the librarians of all well-equipped balloons."

From the *Times* of March 13: a gratifying instance of an almost correctly applied scientific metaphor:—

> Mr. Austin Taylor (Liverpool, East Toxteth), who seconded the resolution, associated himself with the expressions of satisfaction which fell from the mover at the restoration of the leader of the Opposition to his accustomed place in their deliberations. (Cheers.) Now, for the first time, they were confronted by an Opposition which, on the question of tariff reform, was, as regarded its leaders, a united Opposition. It was a good thing for the party led by these leaders and a good thing for the State. He hoped their union was perfect—as perfect as that of the famous double star Algol, whose dual nature could only be detected by the fact that one member of the system periodically eclipsed the other. (Loud laughter.)

The following brief extracts from an article in the *Daily Telegraph* of April 16 show us that astrology still survives in unexpected places:—

> With the death of Dr. Richard Garnett has come the disclosure of the alleged fact that he was a keen student of astrology, if not a firm believer in that vain superstition. . . . . Briefly, the evidence for identifying the late Dr. Garnett with a writer who enjoyed a certain fame in what may be called astrological circles is as follows. The writer referred to signed his articles with the name "A. G. Trent," which is simply an anagram of "Garnett." His most notable contribution to astrological literature was an article published in the *University Magazine* in the year 1880, entitled "The Soul and the Stars," in thich he claimed to have made out a "primâ-facie case" for a certain influence on the moral and intellectual character by the positions of the heavenly bodies at the time of a person's birth.

And towards the end of the article occurs the following:—

> Many good men and true believed in the influence of the stars. Dryden ca[illegible] the horoscope of his sons and made a remarkable prediction respecting one [illegible] them which was verified. Robert Burton, author of 'The Anatomy [illegible] Melancholy,' calculated his own nativity, and foretold the time of his death, which, "being exact, several of the students did not forbear to whisper among themselves that, rather than there should be a mistake in the calculation, he sent his soul to heaven through a slip about his neck."

The following cuttings from the *Morning Post* of April 13 may be profitably compared with other eclipse observations:—

> Naples, April 12, Afternoon.
>
> The sun is still hidden by dense clouds of light red dust, through which it shines as during an eclipse. The disc appears about the size of the moon, and the edge seems pale blue in colour.
>
> * * * * * *
>
> Near the new crater the boiling noise was awful, eclipsing that from the summit. From the lower crater the lava flowed out at the rate of 72 mètres an hour.

The following letters were received about the time of the opening of the Yerkes Observatory, and deserve a wider circulation than that for which their writer modestly intended them. It is hoped that the lapse of time has been sufficient to eliminate any risk of impropriety in printing them.

Esteemed Sir, Belvidere, Ill., Oct. 12th, 1897.

While sitting recently at the table of Mr. ——, a member of the family who are the largest landowners on the Big Foot

Prairie, I heard the suggestion that I should be present at the dedication of the great observatory, a remark based on the gentleman's knowledge of my past benefit to Lake Geneva in its incipiency as a resort. For a person to suggest the invitation of himself would be an act of impropriety unless some special reason is apparent, or his presence may result in benefit to the occasion.

The benefit to Lake Geneva as a resort consisted in part in my decorating parks, as an act of artistic culture, with most elaborate floral and foliage designs representing different nations. Society, at the time, was most gratified and socially most grateful. It was an act of enhancement which helped in causing the opulent of Chicago to conclude that Lake Geneva was to be a permanent resort. The English design in Fontana Park at the head of the Lake was an especial favorite—a Plantagenet lion in tawny colors.

The benefit which my attendance at the dedication of the Yerkes Observatory might be to the occasion, consists in the fact that I am the possessor of a white crow—*Corvus Albus*, which might be placed somewhere in the observatory during the time.

As this statement sent to many might indicate mental aberration or an imposition, it is a gratification to be writing to a gentleman of attainments in science, who may at least know that there is such a thing as an albino crow. The specimen in my possession was hatched out white in a nest with black crows almost within distant sight of the great observatory. I saw it when it was alive. It died when rather young, and it is now mounted and appears most life-like, especially when swinging on its perch. If I should be with this natural wonder, close by the wall in one of the halls, or even in one of the adjoining rooms frequented, the sight of the white crow might aid in entertaining the distinguished guests, while waiting their turn for the view through the largest window looking out upon the universe.

It would not greatly matter whether I should be placed where I could hear the addresses, for these could be read when published; nor should I expect to look into the telescope on the opening occasion. This *rara avis*, ruby eyes, white feet, white beak, and every feather as the snow—what is there else which, for attention, would so compete with the stars?

In my possession are a number of letters received from eminent men.

Begging your pardon for taking your valuable time at this hour,

I have the honor, Sir, to be,

Yours very truly,

————.

ESTEEMED SIR, Walworth, Wis., Aug. 4th, 1898.

You may recall the fact that an offer was made to you, from Belvidere, to place in view in the Observatory a mounted white crow, *Corvus Albus*, to add interest, if it might be, to the occasion of the dedication of the great window looking out upon the universe.

It has occurred to me that in addition to this good wish and intention, I may possess reasons of sentiment which, in the opinion of gentlemen of scientific tastes, might entitle me to a view through the great telescope.

When a youth of thirteen I saw some students congregated looking through a telescope standing on a street corner by the residence of Prof. ——, later of Cornell University and of the University of Syracuse. Waiting my turn I knelt and unexpectedly beheld the most glorious sight afforded by the heavens, the planet Saturn hanging forth in the evening sky in placid splendor. This sight was my inspiration towards higher education; the next term found me a student of science—mentally drawn upward by the influence of the planet Saturn.

Later in life there occurred an astronomic incident in kind, if not so kindly. In the year 1878 I was residing at Lake Geneva with my mother's aunt, Mrs. ——, a New York city lady who had been the first preceptress of the first female seminary established in St. Louis, and who had received an autograph letter from Henry Clay. One evening, late in autumn, we were walking home when the old lady looked up and asked, "Where is the Scorpion?" "It has set," I replied. "No, for I saw it a few nights since."

Time passing more rapidly, seemingly, in advanced age caused her to think she had seen recently what had probably not been discernible in more than a month at that hour in the evening.

"You need not tell me," she said, "I studied astronomy before you were born."

"Yes, but you have had so long a time to fill up your memory that it is no wonder that you have begun to spill over into forgetfulness. Scorpio is one of the summer constellations far in the south, and of brief duration."

Still searching the southern skies she said that I accused her of losing her mind and called me "impudent."

For this I returned, in my turn at asperity, that I might be so, but I would give her a dollar if she could find Scorpio.

"I do not want your money," she replied sharply, and never yielded that the constellation was not visible, only that her dim eyes had failed to locate this zodiacal group.

And this was the "highest toned" quarrel that Lake Geneva ever had—we quarrelled over the stars.

Now in the southern sky do I behold coincidence,—Saturn is in Scorpio, and from its size and brightness I should presume it is not far from its nearest distance from the Earth.

Begging your pardon, I ask if in your gracious favor, there may be an evening named when I may glance into the great telescope, not necessarily at Saturn, but at any star at which the famed instrument may be pointed; since I, at least, exhibited a willingness to add to the interest of your opening occasion.

I have the honor, Sir, to be,
Your very obedient servant,

——————.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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VOL. XXIX. JUNE, 1906. No. 371.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1906 May 11.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Lewis. Sixty-six presents have been received since the last meeting, including amongst others:—

Publications of the Astronomical Laboratory, Groningen, Nos. 15, 16 (De Sitter, Photographic Parallax Tables, and Kapteyn, Trigonometrical Formulæ), presented by the Laboratory; G. W. Hill, 'Collected Mathematical Works,' vol. iii., presented by the Author; Oxford University Observatory, Miscellaneous Papers, vol. ii., presented by the Savilian Professor of Astronomy; 'Radcliffe Catalogue of Stars for 1900' (A. A. Rambaut), presented by the Radcliffe Trustees; 18 Charts of the Astrographic Chart of the Heavens, presented by the Royal Observatory, Greenwich; and 10 Charts presented by the San Fernando Observatory.

The President. I have to announce to the Fellows present that this afternoon the Council elected Mrs. Fleming, of the Harvard College Observatory, an Honorary Member of this Society. Mrs. Fleming's most valuable work in connection with the photographs of stellar spectra taken at Harvard, and her numerous discoveries of variable and new stars, are so well known to you that it is unnecessary for me to dwell upon them. I am sure you will heartily welcome her election as Honorary Member.

I have also an announcement to make with reference to our valued Secretary, Mr. Whittaker. As you all know, Mr. Whittaker was appointed a short time ago the Royal Astronomer for Ireland. Unfortunately this necessitates his absence from this country and renders it impossible for him to carry on his duties as Secretary. With great regret the Council this afternoon accepted his resignation of that position. We are all greatly indebted to him for the admirable way in which he has performed his duties as our Secretary; and I am sure we all deeply regret that necessity should have arisen for his giving up that office. At the same time we heartily wish him the greatest possible success in his new sphere of duty. In accordance with Bye-law No. 7, it has been the duty of the Council to elect one of their body as a successor to Mr. Whittaker, to hold office during the remainder of the current session—that is, until the next February Meeting; and the choice has unanimously fallen upon Mr. Saunder, who has, I am glad to say, accepted the position.

Mr. Whittaker quitted the Chair on the President's left hand, which *Mr. S. A. Saunder* then occupied amid acclamation.

Mr. Lewis being called on by the President to read his paper "On some Points arising out of a Discussion of the Double Stars in Struve's 'Mensuræ Micrometricæ'" and the paper by *Messrs. Furner and Storey*, said: Volume lvi. of the Society's Memoirs will shortly be ready for issue to the Fellows. In that memoir the measures of each of the double stars in Struve's 'Mensuræ Micrometricæ' are collected and discussed. It is a large and homogeneous catalogue, containing some 3000 double stars found and measured by W. Struve in the early part of last century. The varied material it yields may therefore be fairly used in general discussions.

Double stars may be disposed into two classes:—

a. Those in which no relative motion is shown.
b. Those in which relative motion, of some kind, is shown.

The stars discussed range from the North Pole to −15° declination, and may be assumed not to be fainter than the ninth magnitude. The first question which naturally arises is—How are these stars distributed in space? Is the law of distribution similar to that of stars in general?

Mr. Lewis then put on the screen a slide showing the distribution of 405,000 stars in Argelander's Catalogue contained in the same limits as those in the 'Mensuræ Micrometricæ,' and also the distribution of the double stars in the 'Mensuræ,' for each hour of R.A., and said that the wonderful parallelism of the two curves was merely what might have been anticipated, for where the actual number of stars is greatest we should expect the largest number of doubles. If, however, the actual numbers were r placed by relative ones—for instance, the proportion of th relatively fixed to those showing relative motion,—on the wh we should expect these classes evenly distributed, *i. e.*, the cu

$\frac{\text{Fixed}}{\text{Moving}}$ should approximate to a straight line. The real state of things is shown [Slide], where

the average value of $\frac{F}{M}$ from R.A. 2^h to 14^h is 3·1,
" " " " 14 to 2 is 1·9.

There should be a physical explanation of this curve. The two peaks in the Argelander curve are, of course, due to the Milky Way, and as they do not in the least affect the present curve we need not seek a solution there.

The system Σ 1426—which consists of AB, a slow binary, with separation 0″·74; AC, a fixed pair, with separation 7″·70; AD, a third pair, with separation 34″·40—was then considered. With the aid of a six- or seven-inch refractor the close pair may be seen separated; but imagine the triple system removed to four times its present distance from us, so that AB no longer appears as a double but as a single star: AC becomes the close pair, with separation 1″·92; AD becomes an ordinary fixed pair at 8″·60.

The moving companion is still there, but invisible to us, and the system is noted as fixed. This expressed in other words means—The fact that a star having a companion revolving about it, in a greater or less period, cannot be dependent on its nearness or remoteness from our system; but whether we are able to see the satellite does so depend, and hence the relation $\frac{\text{Fixed}}{\text{Moving}}$ may, in the main, be fairly considered a function of the distance of the double star from us.

The explanation had been adopted, not merely because of the failure of various other suggestions, but because the same idea could be derived in other ways. For example, if we are looking at a football match from some point in the field, the various movements of the players, both in velocity and direction, are noted separately and distinctly, which distinctness fades as our position recedes. The scale of motion becomes less and may indeed be used as a rough guide to the distance separating us. Similarly, the scale of movement indicates roughly the relative distances of groups of stars. For other purposes Mr. Lewis had occasion to draw the curves representing the proper motions of the two classes of double stars, that is of pairs relatively fixed, and of pairs in which a relative motion is shown. The ordinates of motion in the former class extended to 0″·110 in R.A., and 0″·140 in N.P.D.; in the latter to 0″·400 in R.A., and 0″·340 in N.P.D., indicating, on this hypothesis, that the relatively fixed are more distant than those showing relative motion—i. e., *in the same region of the sky.* This led to a slide, in which the values $\frac{F}{M}$ were shown radiating from a centre, giving a rough idea of a section of the cluster of stars in which we are apparently situated, and which appears to be egg-shaped.

The speaker then went on to say that in discussing individual pairs great efforts had been made to bring into use their proper motions as determined by meridian observations. In general these proper motions had been derived from Auwers and Porter, but in many instances Messrs. Furner and Storey had undertaken their determination. These proper motions, in many cases, give the key to the solution of the particular kind of relative motion in a pair of stars, and they have also enabled him to obtain the proper motions of 50 faint stars.

Among the other features of the Introduction to the Memoir described and considered was a table showing the relation between the masses of components of different colours.

When speaking of the orbit of a double star, it is always understood that it is the path of one component about the other, and not about the centre of gravity of the system. To obtain this absolute orbit we have, in general, recourse to meridian observations. It will be seen that from the absolute and relative orbits the relative masses of the two components may be deduced. The cases in which this can be done are few, the whole number being contained in the table; but the results are most striking and far-reaching, The table is arranged in the only order in which any law can be detected; this order is independent of the star-magnitude, but appears in some way bound up with colour or, rather, the relative colours of the components.

Mr. Lewis then read a paper on the masses of the components of the double star 85 Pegasi, by *Messrs. Bowyer and Furner.* In this discussion the meridian observations since 1830 had been used. They showed the motion of the principal star for two complete revolutions previous to its actual discovery as a double. The relative masses deduced from a combination of the meridian and micrometer measures were as 1 to 4. There is a third star near the double which is practically fixed in the sky and from which the positions of 85 Pegasi have been measured. A discussion of these gives the relative masses of the components of the Binary as 1 to 3.

Prof. Turner (explaining a paper by *Miss Winifred Gibson* on "Some Considerations concerning the Number of the Stars"). This paper is rather different from those introduced by Mr. Lewis. Mr. Lewis has been dealing with his own observations, and gave us some very interesting physical deductions from them. Miss Gibson, under the advice and direction of Prof. Karl Pearson, takes various observations which have been collected by Prof. Newcomb, especially a list of 72 stellar parallaxes, and attempts to express numerically from a study of them various facts about the distribution of stars. I do not know how familiar to the Fellows of this Society is the sort of work that Prof. Karl Pearson has been doing lately. He has been, if I may attempt a brief statement, considering the "correlation," as he calls it, between different quantities, expressing numerically in the form of a definite number what that amount of relationship is. The numbers vary between 0 and unity. For things very closely correlated the coefficient would be very nearly unity. For instance, the lengths of a man's two arms would go very nearly together. If one arm was long, the other would be long, and they would show a coefficient of correlation very near unity—not *quite* unity, because one arm would be sometimes a little longer than the other, and such cases would diminish the coefficient. On the other hand, you might have two quantities entirely unrelated, as the length of his arms and (say) the length of his purse; and one would find the number representing correlation very near zero; not *quite* zero, because of accidental coincidences. (Laughter.) Prof. Pearson has now studied a very great many cases of correlation, and he finds that if the coefficient is anything between 0 and ½ it is very likely to be accidental. He does not begin to attach much importance to relationships which do not have a coefficient greater than 0·5. One reason for the large values of these mere coincidences lies, of course, in the limited quantity of material that is, generally speaking, available; and it is another aim of Prof. Pearson's work to devise methods of making the best use of a limited quantity of material. Of course any of these relationships would be clearly manifested if we could only get an unlimited number of observations; but having only a limited number, it is not so easy. And this case of determinations of parallax, which we know are limited in number, is a good example. First of all Miss Gibson considers one or two simple assumptions: assuming that there is a correlation between parallax and magnitude (that is to say, that the bright stars are nearer, or even further away), then she finds the coefficient 0·103, which is very low, and means that there is not much connection between the two; indeed, the actual result shown is paradoxical, because, in so far as it indicates anything, it means that the bright stars are further away—which shows what anomalies can be introduced by the limitation of material. Then, again, Miss Gibson takes the connection between parallax and proper motion first of all in R.A. and secondly in declination, which are, according to our experience, much more likely to be related: one comes out at 0·44 and the other at 0·41. That is a point in the paper worthy of note, because these are less than we might reasonably have expected: most of us would have been prepared to find the relationship between parallax and proper motion a pretty close one; but it appears that it is not so close as we might expect. Another way of putting that fact is this:—If the relationship were really a close one, from knowledge of one of the quantities you could predict the other. From the length of a man's right arm you could predict closely the left; and similarly you could in other cases make a formula for prediction of one quantity from others related to it, the success of which would depend on the magnitude of the correlations. But that such prediction is not a success in the case of parallax is shown by just tabulating the actual facts against the expectation. (Here the figures were given on the blackboard.) Then the paper goes on to make rather more complex assumptions; but perhaps I have said enough to show the general character of it, and the rest you will be able better to read in print. It seems to me an interesting contribution from a worker who is not perhaps familiar with our general astronomical methods, but who approaches a most important question from a totally new point of view.

The President. I think the authors of these three papers are to be congratulated on having completed important pieces of work. Mr. Lewis's memoir, which I hope will soon be in the hands of all the Fellows, is really a monumental undertaking. It is not merely a most admirable work of reference, so far as it records completely all the important measures that have been made of the Struve stars, but it is arranged in such a way, and gives such details as to very greatly widen the interest of double-star astronomy. (Cheers.) I think that anyone who will take the trouble to go through the memoir, and particularly to read the introduction, which Mr. Lewis has summarized very briefly today, will find that double-star observations constitute really a very important section of modern astronomical work. It is not so very long ago that the carrying-out of double-star observations at our chief observatories was thought by many to be rather a sort of by-product, and not a matter bearing very largely on the more important fundamental duties of institutions of that class. The facts Mr. Lewis has brought forward, and particularly the facts which Mr. Bowyer and Mr. Furner have utilized, show, however, that double-star observations have really a very important bearing on meridian observations. Of course it is not by any means in all cases that the meridian observations will help the double-star observer in determining orbits. In meridian observations of a double star, what is observed is the centre of luminosity, if I may call it so. In any pair where the stars are approximately equal in mass and also approximately equal in brightness, the centre of luminosity will practically agree with the centre of gravity of the system, and there will be no disturbance of the meridian observation due to the relative motion of the two components. Again, when the stars are unequal in magnitude, and the small star has also a small mass, the brighter star will not be appreciably disturbed by the motion of the smaller star, and no effect will be produced upon the meridian observations. But when the pair consists of stars of widely differing magnitudes and the star of small luminosity is of great mass, as it was in the instances examined by Mr. Lewis, we have as a result a disturbance of the brighter star which has an important effect on the observations by the meridian transit-circle. The fact that

Mr. Bowyer and Mr. Furner have been able to utilize the records of the meridian observations at Greenwich to assist in determining the orbit of the binary dealt with in their paper is, I think, a point of very great interest indeed.

The paper of which Prof. Turner has been so kind as to give an account is also an important one, but, as he has said, I think it is one that will have to be read before it can be completely appreciated. But the facts he has put before us are of high interest.

Mr. Hinks. If I understand Mr. Lewis aright, the very interesting result which he has obtained may be put thus: If we draw a curve with R.A. as abscissa, and the whole number of stars down to an assigned magnitude in that R.A. as ordinate, we obtain two peaks, about 5^h and 19^h, which are due to the Milky Way. If we plot the curve for Struve's double stars only, we obtain a very similar result, which might at first sight be interpreted as meaning that double stars are more numerous about the Milky Way. But if we plot a third curve having as ordinates the ratios of binary to fixed double stars in different R.A.'s, the peak in 19^h disappears, and that in 5^h is left. Assuming that the ratio of binary to fixed is an indication of distance, as Mr. Lewis does, one might be tempted to interpret this as indicating that the Milky Way is nearer in the direction of 5^h than of 19^h, and any such indication would clearly be of very great importance. I should like therefore to ask whether Mr. Lewis has discussed the distribution in declination also, since this might indicate whether the aggregation of binaries about 5^h has really anything to do with the Milky Way, or whether the coincidence in R.A. is fortuitous.

I should like also to ask whether the stars in Sturve can be taken as a fair representation of the double stars known now; whether on the whole this selection is an average selection, so that laws based upon the discussion of Struve would represent what we should expect to find in discussing all the double stars known.

Prof. Dyson. Before saying a word or two about the paper Prof. Turner has summarized I should like to congratulate Mr. Lewis heartily on the work he has finished. For my own part I do not think extreme importance ought to be attached to refined statistical discussions unless there is a good deal of knowledge of the thing dealt with as well. These 72 parallaxes, as I understand it, are taken from the table given by Prof. Newcomb. Now stars have been observed to determine their parallax on two grounds: either because they are bright or because of their large proper motion. It seems to me that no discussion of that Table can be valuable unless these two facts are taken into careful account. If anyone, not an astronomer, discusses astronomical questions statistically there is always, I think, a great danger of falling into mistakes of that kind.

I know that an astronomer might very well make mistakes by not knowing how to discuss statistics properly; and it is extremely important and desirable that we should learn the best, the most reliable, and the soundest methods of discussing statistics: if we have not that knowledge it seems to me the best way which most of us can employ to make up for it is by being very careful in the tabular manner in which we present our results; and I think by taking great care of the tabular way in which we present our results we can very largely—I do not say entirely make up for the more recondite knowledge of statistical methods,—but we can, at any rate, get at the main facts derivable from our observations. I say this, of course, without knowing fully the contents of the paper beyond the abstract Prof. Turner has been able to give us; and one needs longer perusal of the paper before being able to offer criticism with any degree of certainty.

Mr. Maunder. I should like to say how heartily I congratule Mr. Lewis on the conclusion of this very great work of his. I was one of his very earliest confidantes when he was entering on it; I won't say how many years ago, or it might appear that neither of us is so young as we think we are. I want to say how very great my pleasure is that this great work has now come out; and especially that it is the R. A. S. that is now publishing what is the finest double-star catalogue to date. I remember a great many years ago hearing a distinguished astronomer say: "What is the conceivable use of observations of double stars? There are any number of them; and all you can do now is to find out if the smaller star goes round the larger." Well, Mr. Lewis has been able to show that double-star astronomy affords a very important contribution to the study of the general arrangement of the stellar universe for one thing; for another, that it throws some little light upon what we may call the physical condition of stars. The little table he showed, in which the colours of double stars were associated with their masses, I think is one very suggestive from the physical point of view. Perhaps a yet more important result—and a most appropriate one, seeing that many of the observations have been made at a great fundamental observatory—is that Mr. Lewis has been able to show that double-star work has something important to say with regard to the observation of the positions of stars, and so it contributes to fundamental astronomy.

Prof. Turner. I hold no brief in regard to Prof. Pearson's work; but in respect to what has fallen from Prof. Dyson I should be sorry if anyone were to think for a moment that in such work the workers were unconscious of the difficulty to which Prof. Dyson alluded. It is the difficulty of, roughly speaking, the physician, who has to consider disease in relation to the whole human race from such material as presents itself, and that material consists almost entirely of unhealthy people; and he has to make the best of that imperfect material in getting at the very best rules he can for the guidance of both healthy and unhealthy people. But what is he to do? It is the only material he can get; and is he therefore to refrain from discussing it?

I have no doubt the material available for the discussion of parallaxes is selected material, and has all the disadvantages appertaining to selected material; but I do not think that leads to the conclusion that we must not discuss it the best way we can.

Having had to speak about another matter, I have not till now had an opportunity of adding my congratulations to those of others to Mr. Lewis on the completion of this great work, which I am sure we shall all be proud to see in our Memoirs.

Mr. Lewis. With respect to the questions that Mr. Hinks put. Firstly, whether the stars in the Struve Catalogue may be taken as representative of double stars. They may: because these stars range from stars under half a second separation up to, and over, 30 seconds apart; and also they are not selected, that is to say, Struve examined with that one telescope 120,000 stars and noted all the doubles he saw. The other point he raised, as to the effect of dividing the groups into zones of N.P.D., is most important. If, however, you attempt to parcel them into little groups you immediately, unfortunately, so reduce the numbers that they are not quite reliable. But in order to compare them with the Groombridge Catalogue I have actually divided them up into two, viz. from the Pole to 50° N.P.D. and then from 50° to 105° N.P.D. The curves come very much the same, that is, relatively. And I may say the same general result is obtained from a discussion of the Otto Struve Catalogue. And if you take the Burnham Catalogue, which runs further south, you obtain another curve not quite the same as this, but practically parallel. These catalogues being independent of the Struve Catalogue and the material not so good, I have not included them in the general discussion, the main claim of which is homogeneity.

The Astronomer Royal. It may be remembered that at the November Meeting of the Society a preliminary paper was read which gave provisional results of the measures of the photographs of the 6th and 7th satellites of Jupiter that had been made up to that date, November 7. Since then similar photographs have been taken whenever weather would permit; and the photograph now on the screen shows in diagrammatic form the dates of these photographs, which ended on February 15 in the case of the 6th satellite, and on January 26 for the seventh. Unfortunately, in the case of the seventh satellite, observations are somewhat hampered by the circumstances that the satellite moves in an orbit very little open, and, of course, the satellite can therefore hardly be photographed when it is in the immediate neighbourhood of Jupiter, so that during some part of its period the satellite is too near Jupiter to be photographed. It was mentioned in the preliminary paper that the measures of position-angle and of distance were merely given as provisional, and that it was intended to get more accurate positions by measuring them with comparison stars as faint as could be photographed with the astrographic telescope, and that the measures on the photographs taken with the 30-inch refractor would be connected with the reference-stars obtained from the Astronomische Gesellschaft Catalogue by means of plates taken with the astrographic telescope; the suggestion was made by Prof. Dyson, and its adoption has greatly increased the accuracy of the results.

This slide shows simply the dates of all the observations that have been made. The first photograph of the sixth satellite was obtained on 1905 August 23, and plates were taken after that date whenever possible up to 1906 February 15; the considerable gaps are due to cloudy weather. Then for the seventh satellite the first photograph was obtained on October 22, and this object was followed until November 7. It was picked up again on the other side of Jupiter on November 23, and observed more or less continuously through December till January 26.

The results here given, which have been worked up with great skill and care by Mr. Davidson, show the apparent right ascension and declination of the satellite referred to the reference-stars obtained from the Berlin zones of the Gesellschaft Catalogue, and from them the position-angles and distances from Jupiter have been deduced assuming the tabular place of the planet to be correct. The results are given here. For the sixth satellite we have 80 photographs taken on 36 nights, and for the seventh satellite 19 photographs on 15 nights; and as far as can be judged from examination of these, comparing several photographs which were taken on the same night, the results from the different photographs seem to agree very closely. In the case of several of the photographs taken the exposure was much longer than was needed to obtain the sixth satellite, those photographs having been taken specially for the seventh satellite. An interesting point that Mr. Davidson has found by comparing the places of the satellites when near the centre of the plates with results obtained at a considerable distance from the centre is that the latter are practically as good as the former by using this method of these comparison stars. Part of the scheme was to find the errors of the tabular place of Jupiter by means of photographs taken with the astrographic telescope. It is unfortunate that we did not begin, perhaps, quite so early as we might have done; but still we have got ten photographs on ten nights from which the place of Jupiter, or the correction to the 'Nautical Almanac' place of Jupiter, can be deduced with very great accuracy. The discussion of these results is not quite complete, so that I have not included that subject in the paper; but it is a matter of considerable interest, which I hope to bring before the Society at the next Meeting; but I may say that this method of determining the position of planets and the errors of the tables promises rather greater accuracy than can be obtained from meridian observations. We have, of course, a good series of observations with the transit-circle, and also with the altazimuth used in the meridian, to compare with these photographs, and the net result from the three series of determinations seems to be that the error of the tables of Jupiter is very small; there appears, however, to be a very small error in right ascension, and perhaps a natural corresponding error in declination.

These results will be seen better by-and-by, when printed in tabular form. I may mention that I was anxious to get out the results as soon as possible, and the paper has been rather hurried, so that material might be available for any discussion of the orbits of the satellite.

There is another matter which may interest the Society. I have brought here some photographic prints from the negatives of the solar eclipse, which were exhibited at the Royal Society Soirée. The interest of them is this, that they are taken on some paper used for the reproduction of the astrographic map—thick bromide paper or cartridge. But instead of being printed by contact in the ordinary way (which, after trial, we found was altogether unsatisfactory, as all the fine detail of the negative was unavoidably lost), they have been copied in the camera direct from the original negative, so that there is no risk of injury to the negative. I think they give practically as much detail as transparences. It is always a great difficulty to know how to reproduce eclipse photographs so as not to lose the delicate detail that is shown in the original negative, and of course it is rather difficult to send transparencies. I hope that these may serve the purpose of showing the details for comparison with photographs taken at other places.

The President. Once more we have to congratulate the Astronomer Royal on valuable work done by the staff at Greenwich. We have also to thank him for these beautiful photographs. I had an opportunity of seeing them the other night at the Royal Society Soirée, and I feel sure Fellows will be greatly interested in examining them. I think what the Astronomer Royal has said about the manner in which the delicate detail is reproduced is most fully justified.

Prof. Dyson showed a slide on the screen giving the seismograph records obtained at Edinburgh of the following earthquakes:—

(i) Indian Earthquake, April 4, 1905.
(ii) Earthquake in Siberia, July 23, 1905.
(iii) Earthquake in Calabria, September 8, 1905.
(iv) Earthquake in Greece, November 8, 1905.
(v) San Francisco Earthquake, April 18, 1906.

The preliminary tremors from the San Francisco earthquake reached Edinburgh in seven minutes. These preliminary tremors traverse the Earth and do not come along the surface. The speed at which they travel shows the high degree of rigidity and elasticity of the Earth's core.

The President mentioned a letter in the current number of *Nature*, written by Prof. T. J. J. See, in which it was mentioned that two of the four clocks at Mare Island Observatory were stopped by having their pendulums thrown upon the ledge which carries the scale for measuring the amplitude of the swing. The time of the violent oscillation was thus recorded automatically as $5^h\ 12^m\ 37^s$ Pacific Standard Time, eight hours slow on Greenwich. The pendulum of the other two clocks had their points rubbed against the index-scale of the swing so violently that the edge of the scale was brightened and the time of the clocks deranged by twenty seconds.

Prof. Turner said:—Two communications from Prof. Barnard were announced at the last Meeting, on which I wished to say a word or two, but time did not allow. The first paper, which has since been printed in the 'Monthly Notices' for April, consists of observations of magnitude and position of Nova Geminorum and the small stars surrounding it. The Society may remember there was an interesting question with regard to one of these small stars. A photograph was taken of the region before the appearance of the Nova, which showed a small group of five stars with a little star in the middle. This was taken before the outburst. The little star was on the photograph taken by Prof. Max Wolf. We measured the place of it carefully, and came to the conclusion it was near the Nova, but the distance between the faint star and the place where Nova appeared seemed to be about 5″. That was confirmed in a satisfactory manner by Prof. Barnard later on, who, when Nova became faint, saw the two side by side, and measured the distance between them. He got, however, a larger distance—7 seconds,—and that raised the question as to whether one or both stars were in motion. The paper he now presents throws unexpected light on that question. He was measuring these stars round Nova, and finds Star No. 1 seems to be closing up with Nova. In fact, observations up to now would accord with the view that the Nova was actually in motion, receding from what he calls Star No. 7 and approaching Star No. 1. Of course this is only a suggestion at present, but an interesting one, and Prof. Barnard hopes to go on with the measurements.

The other communication from Prof. Barnard relates to the magnificent enlargements of photographs taken by him, which I hope most of the Fellows have already seen in the Library. He sent a few notes upon them, not to be read as a paper. Here are some enlargements we have of the region. These slides now shown are not actually made from his new photographs, but we have photographs of the same regions taken by him which serve equally well for illustration.

There were then thrown on the screen (1) a remarkable group of nebulæ near ρ Ophiuchi, (2) the Milky Way about θ Ophiuchi, (3 and 4) great star-clouds in Sagittarius, (5) the Milky Way near

M. 11. With reference to No. 1, Prof. Turner said:—In the upper part of the picture the structural details seem to show that certain portions are pushed out towards us. The most remarkable thing, however, is the extraordinary example of nebula which extends southwards under that star. The lowest part of the nebula is better shown in the photographs made specially to show it. On the enlargement upstairs there are three asteroids in the region which I have marked.

Prof. Barnard remarks, in reference to the photograph of the great star-cloud in Sagittarius—and it is interesting to have his opinion on the point—that these dark markings, which might be caused by opaque masses of matter, seem to him to be holes.

Prof. Turner then proceeded to show some photographs of the Mount Wilson Observatory, where Prof. Barnard's latest photographs had been taken, and the surrounding scenery.

A Fellow. Has anything been heard of these Californian observatories since the earthquake?

Prof. Turner. I heard from Prof. Hale two days ago, and he said that practically no damage has been done. As regards the Lick Observatory, too, we hear that it has not suffered.

Mr. T. E. Heath showed on the screen some copies of star-maps in which each star was represented by a green and red disk. These were designed to be viewed through spectacles with a red and green glass, examples of which were provided for the use of those present. It was expected, and the experiment proved successful in many cases, that the two coloured star-disks would be combined as in a stereoscope.

The following papers were announced and partly read:—

T. Lewis. "Some Points arising out of a Discussion of the Double Stars in Struve's 'Mensuræ Micrometricæ.'"

W. Bowyer and H. Furner. "On the Orbit and Mass of 85 Pegasi."

John Tebbutt. "Observations of Uranus at Windsor, New South Wales."

Natal Observatory. "Observations of Comet *c* 1905."

B. L. Newkirk. "Note on the Parallax and Proper Motion of the Central Star in the Annular Nebula in Lyra."

S. Hirayama. "On the Ratios of the Triangles in the Determination of the Elliptic Orbit from Three Observations."

Winifred Gibson. "Some Considerations concerning the Number of the Stars."

E. Nevill. "On the Ancient Eclipses of the Sun."

A. Stanley Williams. "Elements of Five Long-Period Variable Stars."

Rev. T. E. Espin. "New Double Stars."

W. F. Denning. "Observations of Jupiter in 1903 and 1905–6."

Royal Observatory, Greenwich. "Observations of Jupiter's Sixth and Seventh Satellites from Photographs taken with the 30-inch Reflector of the Thompson Equatorial." (Communicated by the *Astronomer Royal.*)

The following gentlemen were elected Fellows of the Society :—

Edmund Dickson, F.G.S., 2 Starkie Street, Preston, Lancashire.

Jacob Halm, Ph.D., Royal Observatory, Blackford Hill, Edinburgh.

John William Hicks, Ordnance Committee Office, Woolwich.

William Malin Hunt, 48–50 London Road, Nottingham.

Edwin Baikie Simpson-Baikie, Lieut. R.N.R., 142 A, The Bluff, Yokohama, Japan.

The following Candidates were proposed for election as Fellows of the Society :—

Arthur Grant Stillhamer, Yerkes Observatory, Williams Bay, Wisconsin, U.S.A. (proposed by S. W. Burnham).

Ralph Falcon, M.A.Oxon., Barrister-at-Law, Camerton Hall, Workington, Cumberland (proposed by Cecil G. Dolmage).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1906 April 25.

A. C. D. Crommelin, B.A., *President*, in the Chair.

Secretaries : J. A. Hardcastle.
J. G. Petrie.

The Minutes of the previous Meeting were read and confirmed.

The names of four Candidates for Membership were read and passed for suspension, and the election by the Council of three new Members was unanimously confirmed.

The President announced that the date of the next Meeting was unavoidably altered, as the 30th of May, which was the proper date, was the day appointed for the Visitation of the Royal Observatory at Greenwich; and as this would mean the absence of a considerable number of Members of the Council and other Members of the Association, it had been decided to hold the Meeting a week earlier, viz. on the 23rd May.

The President then referred to the death of Sir Gordon Miller, which had taken place very suddenly the previous day. He reminded the Meeting that Sir Gordon Miller had acted as one of the Auditors of the Association for a number of years, and he was sure the Members generally would desire to join with the Council in expressing their condolences with his family.

Mr. Edwin Holmes read a paper entitled "Notes and Queries incidental to Mr. Maunder's Researches." Among the points raised by Mr. Holmes were the frequent statements by Mr. Maunder in his papers that when certain spots reached the central meridian a magnetic storm instantaneously began, while in other instances a period of 26 hours elapsed before the outbreak. He was not sure which theory Mr. Maunder held, but the facts appeared to him to be irreconcilable. Assuming the period to be 26 hours, and that the magnetic stream consisted of material particles, the velocity of 1000 miles per second was difficult of belief. Had we any knowledge of actual motion of matter at such a speed? The return of a meridian on the Sun after a rotation appeared to involve a fallacy, as owing to the varying velocity of different parts of the solar surface such a meridian on its return to the centre would no longer be a straight line but a strongly curved one.

Mr. A. Hutt maintained that we must regard the Sun as a whole as one magnet and the Earth as a whole as another magnet. It was contrary to all experience of magnetism to think of the Sun's magnetic influence otherwise than as emanating from its centre and as primarily affecting the Earth's centre. It appeared to him to be absolutely grotesque to imagine a certain portion of the Sun's surface discharging these influences without any target of attraction.

Mr. Maunder said that if Mr. Holmes had read his papers a little more carefully he would not have found it necessary to raise the questions he had. Some of the things that Mr. Holmes had put down to him, he (Mr. Maunder) had not been able to find in his papers at all—indeed, they were distinctly contradicted by them. With regard to "definite meridians," he had simply, adopting Carrington's formula, computed the heliographic longitude of the centre of the Sun's disc for the time of commencement of each disturbance, and had pointed out a fact which became obvious directly this computation was made, viz., that there was a strongly-marked tendency when a magnetic disturbance had occurred for another to follow when the same longitude had returned to the centre of the Sun's apparent disc. He had expressed no opinion as to the connection in point of time between solar disturbances and magnetic storms on the Earth. It was Prof. Riccò who had drawn from the table he had published the period of 26 hours. He (Mr. Maunder) would like to have a great deal more light on the subject before he committed himself to the statement that we knew how long the influence took to travel from the Sun to the Earth. With regard to Mr. Hutt's remarks, he (Mr. Maunder) thought that gentleman had made some rather rash assumptions as to the nature of the Sun's magnetic action.

Mr. Maunder then exhibited a number of lantern-slides dealing with the subject of Mr. Holmes's paper.

The President said that Mr. Maunder's results on the connection of magnetic disturbances with the Sun's rotation were of such an important character that he was sure he himself would welcome the free discussion of any possible objections that could be offered to them; and anyone who thought he saw any weak points in his conclusions would be helping the establishment of the truth by putting them forward and asking for Mr. Maunder's opinion upon them. He hoped everyone would feel that Mr. Maunder had satisfactorily removed the particular difficulties that had been raised about his conclusions. Of course, the fact that the Sun did not rotate as a rigid body necessarily made the period a little more difficult to define, and made it less likely that there was one single period. They had rather a number of periods within a limited range, so that they must not be surprised if they found some discordance in the periods deduced from different groups.

The President then asked Mr. Cowell to give an account of his work on ancient eclipses, and the results as regarded the motion and acceleration of the Sun and Moon.

Mr. P. H. Cowell said (I.) he had found the tables of the Sun and Moon could be so altered as to make the calculated ancient solar eclipses agree with the historical records. It had previously been supposed that these solar eclipses had been described with hopeless inaccuracy. (II.) The nature of the alterations required by the eclipses might be put into words thus: "Certain definite rates of diminution are required in the length of the nodical month and nodical year." The nodical year meant the time from one passage of the Sun through the node to the next. The question arose, was the tabular place of the node of the Sun to be altered to produce the necessary change? There was reason for answering this question by ascribing the change to the Sun. The inference from eclipses now took the more popular form: "Certain definite rates of diminution in the length of the month and year as measured in days are indicated by the eclipses." (III.) Many physical explanations might be suggested. The one put forward tentatively at the last Meeting of the Royal Astronomical Society was as follows:—The tides are to be supposed to increase the length of the day to the extent of $0^{\mathrm{s}}\cdot005$ in a century. This would produce an apparent acceleration of the Sun of the amount required by the eclipses, but at first sight would seem to produce an acceleration of the Moon ten times too large. But the spin lost by the Earth would be taken up by the Moon, or, in other words, by the principle of conservation of angular momentum, the Moon would be getting further away and moving more slowly. It was quite possible that nine-tenths of the apparent acceleration might be thus counteracted, leaving one-tenth as required. These results have been divided into three sections. Each section presupposes the acceptance of the previous sections; but it may be pointed out that, on the other hand, each section is independent of the subsequent sections. In particular, it is possible to hold that a diminution in the length of the year as measured in days has been proved, without accepting the suggested cause. The observational evidence for Section I. was therefore of supreme importance. The case rested upon the unanimous evidence of six solar eclipses, and was supported by ten lunar eclipses.

Mr. Lynn asked whether any more had been heard of the eclipse of B.C. 1063, as he understood that Mr. King, of the British Museum (who had deciphered the tablets on which it was recorded), had published, or was about to publish, a pamphlet giving full particulars about it.

Mr. Cowell answered in the negative.

The President said Mr. Cowell deserved the thanks of the Association for bringing this important subject before them. Many of them had seen the course of papers that he had been bringing out during the last year before the Royal Astronomical Society. Although so many had worked at it no one had succeeded in getting a theory that would make all the ancient solar eclipses fit, because, as Mr. Cowell said, they had tried to find only a single unknown quantity—that was the secular acceleration of the Moon. Mr. Cowell, by bringing in a second unknown quantity, viz. the solar acceleration, had been able to make them all fit. This great triumph gave a strong presumption of reality to the hypothesis that the year as measured in days was diminishing. That, of course, was a very startling result. At first sight it seemed to imply that the Earth was continually approaching the Sun. However, the explanation Mr. Cowell now brought forward was that the period of the Earth round the Sun had not altered sensibly, but that it was our day that was longer. And Mr. Cowell had brought forward what they might call a *vera causa* to explain this—a cause which they knew was in action and which had already been considered, although its effect on the Sun had never been considered before.

The Rev. T. E. R. Phillips read a paper by *Mr. W. H. S. Monck* on "Ancient Eclipses," the *President* explaining that this had really been answered in anticipation by Mr. Cowell.

Mr. Cowell then described in detail the eclipses on which he had based his calculation, taking them in the order of their antiquity.

Mr. Lynn said that, as he had been specially alluded to in Mr. Monck's paper, he would like to say that the eclipses recorded on Assyrian (not Babylonian) tablets, to which he had referred in his book on "Remarkable Eclipses," and in other places, were within the era of the Eponym Canon of George Smith, and their dates were given according to the years of the reigns of the Kings during which they occurred.

The Meeting then adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

The first of the Afternoon Meetings for the present Session was held on Wednesday, May 16, in the Society's Rooms, 70 Victoria Street, Westminster, *Mr. Richard Bentley, F.S.A.*, President, in the Chair.

Dr. W. N. Shaw, F.R.S., read a paper, which he had prepared in conjunction with *Mr. G. C. Simpson*, on "An Instrument for Testing and Adjusting the Campbell-Stokes Sunshine-Recorder." Experience has shown the necessity of an instrument for testing the shape and dimensions of recorders, and for verifying their adjustment when installed. But it is not at all easy by mere inspection or simple measurements with ordinary measuring-instruments to check the adjustment, nor is it possible on a sunless day, without some special instrument, to check the orientation, and so the time-scale of the sunshine-recorder. The authors have devised an instrument for this purpose, which they fully described in the paper.

Mr. R. G. R. Lempfert read a paper on "The Development and Progress of the Thunder-squall of February 8, 1906." This squall was first noted at Stornoway soon after midnight, and the last station in England to feel its effects was Hastings, over which it passed at about 4 P.M. The rate of progress was nearly uniform, though it increased somewhat in the south-east of the country, where the thunder- and hail-storms were most intense. The average speed of advance of the line of squall was about 38 miles per hour. The most marked feature of this squall was the sudden shift of the wind in the course of a few minutes from south-west to north-west, and it was during this period that the thunderstorm occurred, accompanied by a rise of barometric pressure and a fall of temperature.

The New Reduction of the Observations of Groombridge.

After a period of diligent activity in business, Stephen Groombridge, at the age of 51, employed an interval of partial relief from business cares in the "laborious amusement" of observing with the transit-circle. This work he kept up continuously for more than nine years, amassing a total of nearly 27,000 observations. These have now been newly reduced by Messrs. Dyson and Thackeray, who are to be congratulated on the successful and workmanlike completion of their task. Thus the labours of Groombridge are still meeting with ample recognition, justifying his devotion to the object he had in view.

At first sight, the value of Groombridge's work might be considered as impaired because of the restricted area of sky within which he observed, of which the southern limit is 52° N.P.D.

But a large portion of many of the earlier star-catalogues is in effect zodiacal. The motive for the observations was drawn very largely from interest in planetary astronomy. The catalogue of Groombridge helps to restore the balance. Furthermore, the observations of Groombridge, while they include quite fully stars visible to the unassisted eye north of 52° N.P.D., also cover a far greater number of stars of telescopic magnitude, about a thousand of them being fainter than 8·0 mag. This is a fact of great importance, when we consider the stars as objects of interest in themselves, apart from their usefulness as reference points in planetary and geodetic operations. Each star-catalogue of quasi-fundamental character that contains the observed positions of very many faint stars constitutes a medium through which the systematic accuracy attained in computing the motions of the principal stars may be extended to the sources of position for fainter stars, and even to zone-observations themselves.

In the deduction of the right-ascensions of Groombridge, the authors have encountered serious obstacles. Frequently Groombridge noted the time of transit across only one thread, still more frequently only two. The collimation of the instrument is not accurately known. For a large portion of the period of observations there are few or no observations of the transits of standard close polar stars. The attempt of the authors to impart a quasi-fundamental character to the reductions emphasized these difficulties. Possibly an even more useful result might have been secured through a strictly differential treatment of the transits upon the basis of an extensive standard Catalogue; but even in this way the difficulties, in very large measure, would have remained.

I have examined the new right-ascensions for systematic difference from my standard Catalogue ("Positions and Motions of 627 Principal Standard Stars," Astron. Journ. No. 531–2), greatly enlarged to include more than 2000 well-determined stars. Of these, 570 are in common with the Catalogue of Groombridge. The error in the meridian plane of the instrument appears to be quite small; but there appears to be an interesting periodic error which is expressed by the following formula, in the sense of correction to the R.A. of Groombridge:—

$$+0^{s}\cdot047 \sin \alpha + 0^{s}\cdot035 \cos \alpha - (0^{s}\cdot042 \sin \alpha + 0^{s}\cdot027 \cos \alpha) \tan \delta.$$

This seems to mean that the computed polar deviations of the instrument, such as are removed by the correction $n \tan \delta$, are subject to periodic error varying with the R.A. of the stars observed. In the introduction the authors describe the difficulties they encountered in the effort to compute n.

My effort to compute the magnitude equation of Groombridge apparently met with greater success than would have been anticipated. It appears, on what looks like reliable evidence, that Groombridge observed fainter stars relatively later than the brighter at the rate of 0^s·011 for a difference of one magnitude. The mean value of this quantity for all observers appears to be about 0^s·007; so that the equation of Groombridge is rather large.

The newly computed N.P.D.'s for Groombridge must be regarded as a most welcome contribution to astronomy. The reductions in this coordinate are strictly differential. The precision of the results places them in a high rank, close after the more elaborate fundamental determinations of the first half of the nineteenth century. The systematic errors in N.P.D. seem to be of really trifling amount. These facts are all the more surprising when we remember that Groombridge read the zenith-distances by only two microscopes, while he was bothered with the transits at the same time.

The value of the new reduction is much enhanced by the thorough, intelligible, and yet concise exposition of circumstances relative to the work of observation and reduction.

Proper motions for all the stars are found in the Catalogue. Nearly all are computed by the authors from a comparison of the new Groombridge positions with those of late Greenwich observations. From what has already been stated, it may be inferred that the proper motions in N.P.D. enjoy a considerable degree of precision; and while this estimate must be modified in some degree as to the motions in R.A., it is still probable that, in point of precision, they compare favourably with those of Auwers-Bradley for the same area of sky, because of the generally greater number of determinations on which the right-ascensions of far northern stars in Groombridge are founded.

It was quite natural, therefore, that the authors should have felt tempted to employ the very large number of proper motions they had deduced in a new discussion of the solar motion. Former computations have been based chiefly upon the Bradley stars, or upon stars having large proper motions. The Catalogue of Groombridge offers distinctly new material. It is necessary to bear in mind, however, that the effect of precession is so involved with that of solar motion that a mutual indetermination exists whenever the proper motions from which it is designed to derive these quantities are not well distributed over the sphere. The greater the lack of symmetry with the equator in this distribution the greater the indetermination. Other objections occur, such as non-conformable star-drift in restricted areas and the like.

The discussion of precession and solar motion by the authors is preceded by extensive and interesting comparisons of the Groombridge-Greenwich places with those of various star-catalogues. Then follow inquiries as to the distribution of the stars in the area concerned as to magnitudes, motion, and type. It may be worth while to present here in condensed form one point of special interest. Arranging the stars in the order of magnitude and deducing the percentage of stars in each group having centennial motions less than 10″, we have:—

Mag.	No. of stars.	Percentage of motions<10″.	M.
			″
4·0–4·9	163	74	..
5·0–5·9	538	80	3·2
6·0–6·9	1149	88	2·8
7·0–7·9	1369	92	2·2
8·0–	940	96	2·3

It is here evident that the mean proper motion of fifth-magnitude stars is not greatly superior to that for stars of the eighth magnitude. Summary treatment of the table on page xcii of the Catalogue indicates that the ratio of the mean proper motion for 5·0 mag. relatively to that for 8·0 mag. is about 1·5. If we adopt the very probable hypothesis that mean motion is a fairly approximate measure of relative mean distance, for stars in the same quarter of the sky, we must obviously conclude that the mean distance of an eighth-magnitude star is about 1·5 when the mean distance of a fifth-magnitude star is taken as unity, whereas this ratio should be nearly 4, when brightness is accepted as the sole criterion. This disparity is well confirmed by an examination of the values of M appended to the preceding table. M is the angular length of the Sun's path for a century, as seen from the mean distance of stars from which that particular value of M is derived—the secular parallax of those stars. M has been derived by the authors for each group of magnitudes (p. cvii). The relative mean distances of the several groups should then be in the ratio of the reciprocals of M; and this confirms the relation already pointed out. It is trne that motions greater than 20″ were omitted in the derivation of these values of M, and that a greater percentage of these large motions exists in the groups of brighter stars, so that the M's should be relatively larger for the brighter stars. But the correction from this source would not very materially alter the ratios. I have laid stress upon this point because it confirms what has been found in similar discussions elsewhere.

Discussion of the relations of spectral type to area and magnitude of motion seems to confirm certain important conclusions by Pickering and Kapteyn (p. xcv). While space is wanting for comment on this feature of the work, it should be remarked that the authors conclude that stars of Type II. seem to be specially well adapted for use in discussions of solar motion.

As already pointed out, proper motions of stars in a polar cap are not well adapted for deduction of a value of the precession constant. By far the greatest weight for the constant, *m*, is got from stars near the equator, say within 30°. By an ingenious method the authors arrive at a value for *n* for which they adopt 20″·0531, between the corresponding values of Newcomb and Struve. It must be conceded, however, that very little weight attaches to this result.

In dealing with solar motion the method adopted by the authors is that of rectangular coordinates (Airy's), the validity of which has been called in question and actively debated for several years —more recently by Kapteyn. The authors also base their discussion on the precession of Struve; and although they thus avoid complication in their deductions, the final results are necessarily affected by a degree of uncertainty dependent on the correction required for Struve's precessions. However tempting, it is impracticable to follow out here the details of this interesting discussion set forth in the introduction of the Catalogue. Allusion may be made to a few points. Taking generally 4001 stars having centennial motions less than 20″, these coordinates of the solar apex result:—

	A.	D.
Equal weight to each star	275°	38°
Equal weight to equal areas	272	31

Furthermore, grouping the stars according to magnitude, it is found that the values of D, declination of the solar apex, increase from 27° for 5·5 mag. to 43° for 8·5 mag.—to say nothing of the value, 16°, found for the few stars (200) that are brighter than 5·0 mag. This is a fact of much interest, and its existence is not without some favourable indications in previous computations. One would hesitate, however, to accept this as a phenomenon of nature without a much greater weight of testimony in its favour than can now be brought to bear. Yet it is also difficult to offer a readily acceptable explanation of this sequence of numbers based upon the hypothesis that it is wholly due to imperfection in the data of computation. The authors call attention to the very unequal distribution of the Groombridge stars in the different hours of R.A., only one-third of the stars being found between 6^h and 18^h of R.A. (Int. p. xci). This inequality is much greater for the faint stars than for the brighter. It is quite possible that the origin of the discordance is related to this fact, coupled with the further fact that only for the groups fainter than 6·0 mag. is the number of stars sufficient for a fair elimination of that part of the stellar motion that is purely random in character. It is necessary to remember that this elimination should be as complete as possible in each unit of area that forms an element of the computation, and not simply in the totality of stars considered.

The solutions according to spectral type result in part as follows:—

	No. stars.	A.	D.	M.
Type I.	1100	269°	23°	2·4
Type II.	866	273	37	2·8
All others	2035	280	41	2·4

The notable feature here is that M, the mean secular parallax of the group, is not very materially smaller for stars of Type I. (supposed from their aggregation near the Milky Way to be the more distant) than for those of Type II., or of other stars in general. On the other hand, the value of D for stars of Type I. differs rather remarkably from those for the other two classes.

The authors call attention to the smaller values of D which they find from stars having larger proper motions. From 163 stars having centennial proper motions greater than 20″ they find:

$$M = 25''\cdot 1; \quad A = 275\tfrac{1}{2}^\circ; \quad D = 30\tfrac{1}{2}^\circ.$$

Astronomy is in possession of a number of refined determinations of these quantities based upon stars having comparatively large proper motions. The following examples are cited because they relate to stars having mean proper motion not greatly different from (though somewhat greater than) that of the 163 stars employed by the authors, and because the computations, like this, were based on the method of Airy:—

Computer.	No. stars.	M.	A.	D.
Bischoff..........	480	34″	291°	43°
Stumpe..........	430	30	280	40
Porter..........	533	30	281	40

There is here no evidence that large proper motions lead to small values of D. The discordance from the value of D derived from Groombridge stars seems to find adequate explanation in the necessarily small number of stars employed in the computation.

A striking feature of this discussion of solar motion is its approximate agreement with the general results of other computations, wherein the preponderance of stars is found in equatorial areas. We may take as the general result of the computations of the authors: $A = 274^\circ$; $D = 35^\circ$, near the mean between the two results, one taking the star and the other area as the basis of weight. For computations in general we may state the general result: $A = 275^\circ$; $D = 35^\circ$ to 45°.

Some students of the subject put D at 30°, or at an even smaller number. We must, therefore, regard the result obtained by the authors as in satisfactory accord with the general result from other sources. This fact appears to offer remarkable support to the hypothesis that the motions peculiar to the stars themselves (disregarding the apparent parallactic motion, the reflex of solar motion) are, in the broad sense, at random. More specifically this may be defined as meaning that if we take any large fraction of the volume of stars to which these computations relate (such as one-fifth or one-tenth, for example) the resultant of all the purely stellar motions in that volume will be very nearly the same in amount and direction as that for the entire volume containing all the stars under examination. The computation is very sensitive to any deviation from this law, as any one may learn from experience. The authors point out that, for the Groombridge stars, an adopted increment in the amount of general southward drift of the motions equal to 0″·1 in an interval of about 80 years would correspond to an alteration of computed D of 4° (Int. p. cxi).

While this means that the value of D is vitally dependent upon the systematic corrections adopted for the observations, it also seems to mean that, after all due allowances, there can be no very decided difference in the resultant of the motions for Groombridge stars from that pertaining to the general mass of similar classes of stars distributed throughout the entire sphere. **Lewis Boss.**

With the British Association in South Africa.

[Concluded from p. 215.]

But our astronomical interests are being neglected for geological. Let us enquire what has happened to the Members from Groningen, whom we left some time ago determining the brightness of the Milky Way out on the veldt, with the aid of a cigar and a visiting-card. This combination makes a very excellent photometer. You blow up the cigar to a glow, and see how far off the card must be held that its visibility may just be lost against different regions of the sky. The result is most interesting. The ratio of brightness of the Milky Way to other parts is of course greater in a perfect sky than it is in Europe, but it is not so very much greater; which means that the sky has a luminosity of its own not due to stars. Is it a kind of permanent auroral glow, or a luminescence of rarefied gas at low temperature? The question suggested to the Member from Pulkowa another—Why is the snow brighter than the sky on a clear, moonless night far away from artificial light?

The long journey north from Kimberley to Buluwayo was enlivened by a hot axle on the dining-car, which the whole party inspected every half-hour as nearly as the stench of burning castor-oil admitted. And this raised another insoluble question—Why is sulphur the proper remedy for hot axle-boxes? It seems an unlikely cure, but it worked, and got us slowly to Mafeking, where they put on a new pair of wheels in the middle of the night, and enabled us to catch up again trains C and D, who had been nursing the hope that for once they would get there before the "official party." But this they never once did, though they charmed both sweetly and otherwise.

Ten years ago Buluwayo was the seat of Lobengula's kraal. Shortly afterwards it started in life under the impression that it was going to be a big city, requiring plenty of room. So now it is half a mile across an open space from anywhere to anywhere else, and the "suburbs" where Father Goetz lives are very much *in rure.* But here, just as keenly as everywhere else, the Jesuit Fathers are showing their historic devotion to astronomy, and Father Goetz is in charge of a very interesting meteorological equipment, with the beginnings of an astronomical. His first result was to find that the accepted latitude of Buluwayo was seven miles wrong; his next to show that the chill of dawn is a very real thing in Rhodesia. Day after day his thermographs show a sharp fall in the temperature just at sunrise, which has in this country the unpleasant result of coating your camp bath with ice just as you propose to get into it. But what is the reason?

Father Goetz has an extremely interesting form of almucantar, very suitable for the method of finding time and latitude recently described by Mr. Cooke and Mr. Wade. There is a small horizontal telescope with a 60° prism outside and with base next the object-glass; refracting edge horizontal, and just above a saucer of mercury. When a star is about 60° high its light passes through the upper face normally, is totally reflected at the lower, and goes through the lower half of the objective. It also is reflected from the mercury, passes normally through the lower face, is totally reflected at the upper, and passes through the upper half of the objective. When the elevation of the star equals the refracting angle of the prism the two images coincide. It is a very elegant adaptation of M. Lœwy's aberration arrangement with the prism reversed, so that no silvering is required; it is much cheaper than a theodolite, and can be extemporized by anybody with a small telescope and a prism. One wonders how much truth there is in the verdict of an old Chatham friend who classes all these devices as "gims." A gim seems to be a pretty thing that is no improvement on the older things, such as a field method that is only really useful in a fixed latitude, for which special tables can be computed.

At Buluwayo we had Mr. Maciver's lecture upon the ancient ruins of Rhodesia, which were invested by Mr. Theodore Bent with extreme astronomical interest. He made out in them a special decoration facing the solstitial sunrise, and alignments of stones for the meridian observation of northern stars, while all the dimensions of the buildings were occultly connected in regular Pyramid fashion. Zimbabwe was full of astronomical meaning, as we now know is the case with every collection of old stones. But the Zimbabwe myth has fallen on hard times. First of all Mr. Franklin White has proved that Mr. Bent's measurements were all wrong. And now Mr. Maciver declares that the buildings are not the work of Sabaeo-Arabians or Phœnicians, or Himyarites, but that they were made by Kaffirs two or three hundred years ago. Needless to say, the Rhodesians do not intend to take this wrong to their country lying down.

An astronomical delegate to St. Louis has recently encouraged certain Americans in their belief that the Grand Cañon of the Colorado is the finest thing in the world. *He has never seen the Victoria Falls.* No words can give any idea of how splendid and how perfectly delightful they are. The best point about them is that there is nothing to spoil the perfect delight. The sides of the chasm are in most places firm and square and safe. You can lie comfortably on the grass and look down four hundred feet, through the delicious cool blast of air and spray and rainbows.

You can paddle across to Livingstone Island and sit within a foot of where the main fall goes over the edge; and you will want never to come away.

No one who has seen the Falls will regret to hear that they will never be much use for power; the volume of water is too variable. When we were there the water was lower than had been known before, and the Falls all the more beautiful, for they were broken up into a hundred falls, and the spray did not hide them. So there is hope that they may be preserved for ever; and among the many fine things that the Chartered Company have done in a fine way, nothing is better than the way they have guarded the neighbourhood of the Falls from desecration. The grand way of doing things that Mr. Rhodes had has passed to his successors. One of his chief lieutenants is anxious to have some day a first-class observatory in Rhodesia. He was talking of this one day to a rich man, pointing out how men had made their names immortal by endowing observatories; "and the fellow," he said, "began to look rather anxious; he thought I was going to ask *him* for the money." So he proceeded, "But of course we shall have to be very careful. We don't intend to allow any Tom, Dick, or Harry to make himself immortal in this country!" And the last words that we heard at the Victoria Falls Station as we started back on our long journey home were: "Remember what I said about the observatory; we are going to be very careful about our immortals here."

A. R. H.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Morrison, the Astrologer, as an Astronomer.

GENTLEMEN,—

I do not often trouble you about paradoxers, thinking it best to ignore them. But I have recently come across what seems to be a warning of what may be the consequence of taking notice of that race of beings. Morrison used the sobriquet "Zadkiel" for his astrological predictions, which, whether he believed them or not, he found very profitable. For his pseudo-scientific writings he retained his real name. The first of these was 'The Solar System as it is, and not as it is represented,' which appeared in 1857. In 1860 he put forth a *brochure* on the motions of Encke's Comet, which he sent with a letter to the Royal Astronomical Society. In a postscript to the public, at the end of this, he tells us how his 'Solar System' had fared. This was, he states, what might have been expected in such a "miserable, novel-reading, money-grubbing, tuft-hunting generation." His publishers had sent out the full number of copies to reviewers, but only three periodicals had deigned to notice his book. The unkindest cut of all was that several copies had been found uncut, offered below half the publishing price (it seems to me a tenth part would have been more appropriate) at bookstalls. But what I wish to draw attention to is that one of those which *did* review the 'Solar System' was the *Athenæum*, which, Morrison says, "gave a very fair notice, though it cavilled a little, as such writers feel called on to do." That the *Athenæum* should insert a notice of the work in question which pleased the author, I confess puzzled me until I looked at it, when it was easy to see that the supposed praise was ironical. Nevertheless, Morrison at the end of his paper on the comet, printed portions of this review, which gave the appearance of its being altogether favourable. Surely the moral to editors is to leave such writers severely alone.

Morrison thought he had made a great discovery in noticing that if the Sun and solar system are in motion the actual path of a planet or comet is not elliptical but cycloidal. It apparently never occurred to him that (as the *Athenæum* reviewer told him) no one ever supposed that the elliptic motion was other than relative. This the *Athenæum* reviewer plainly told him, which was probably what he meant by "cavilling." One wonders whether Morrison ever hesitated to say when he walked or sailed directly towards a visible object that he did so in a straight line, although the rotation and revolution of the Earth rendered his actual motion in space of a very different kind. The reviewer further pointed out how he had, in the Preface to his 'Solar System,' perverted Sir John Herschel's approval of Pond's remark that a solar motion, "if it have a velocity at all comparable to that of light," must produce a solar aberration, with the assertion that the velocity of the solar motion was equal to that of light. It never occurred to him that the idea was used in the same sense that the Earth's orbital motion produces a sensible aberration, although the amount of that motion is at the rate of only about half as many feet in the time that the waves of light move miles. Whence he got the notion that Newton concealed the MS. of the 'Principia' for many years, "lest offence should be taken by the jealous *savans* of his day," it would be hard to say. The *Athenæum* reviewer reminded him that it was written and printed within less than two years, the expense being borne by Halley, because the Royal Society was at that time short of funds. And then, in a tone of banter, the author is told that the reviewer hopes to help in playing the part of Halley for him, because "there are many who like spicy writing"; and he does it good-naturedly at the risk of being reckoned amongst the "toadies who scribble in scientific journals"—one of Morrison's amenities.

Nothing abashed, however, the latter published another book in 1868, and a second edition of this in 1872, under the title 'The New Principia, or True System of Astronomy, in which the Earth is proved to be the *stationary* centre of the solar system, and the Sun is shown to be only 306,006·5 miles from the Earth.' The distance of the Moon he afterwards determines (less exactly) at 27,304 miles. Perhaps I may be thought, like the *Athenæum* reviewer in 1857 of Morrison's earlier book, to be devoting too much attention to it, but as Horace remarks (1 S. i. 24, 25):—

> .... quamquam ridentem dicere verum
> Quid vetat?

I will therefore conclude by quoting the *whole* of the last chapter (xx.) of this remarkable work, which will not occupy much of your space:—"It is probable that the so-called chemical rays of the Sun are as mythical as its heat rays."

Such was the great astrologer Morrison, *alias* Zadkiel; there is no fear of my hurting his feelings as Swift (under the pseudonym of "Bickerstaff") did those of his predecessor Partridge, for he died in 1874.

Yours faithfully,

Blackheath, 1906, April 21.

W. T. LYNN.

Sosigenes.

GENTLEMEN,—

The correct chronology of the various philosophers of this name, mentioned in your last number (p. 216), is as follows:—

1. A stoic philosopher, a pupil of Antipatros of Tarsus, who, again, was a disciple of the well-known Diogenes of Babylon, who seems to have had a considerable share in making the Greeks acquainted with Babylonian astronomy. This Sosigenes must therefore have lived about B.C. 100.
2. The astronomer who assisted Cæsar in the reform of the calendar in the middle of the first century B.C.
3. A peripatetic philosopher who must have lived about the end of the second century after Christ, as the commentator Alexander of Aphrodisias was his pupil (Brandis, 'Scholia in Aristotelem,' p. 158 b, 29, and p. 741 b, last line). To this Sosigenes we owe our knowledge of the beautiful system of homocentric spheres of Eudoxus, which would otherwise be practically unknown to us.

Armagh Observatory,
1906, May 14.

Yours faithfully,
J. L. E. DREYER.

Some Errata.

GENTLEMEN,—

Through no fault of yours, I had not the opportunity of correcting the report published in your last issue of some remarks which I made upon Jupiter's Satellites at the April Meeting of the Royal Astronomical Society. It would be taking an ephemeral utterance too seriously to publish a considerable list of *errata* to it, but it is only fair to those who may have found it somewhat bewildering to say that some mistakes therein are due to no one person, but are the outcome of conflicting efforts of shorthand-writer, proof-reader, and myself. Yours faithfully,

University of Durham Observatory, R. A. SAMPSON.
1906, May 8.

[We regret that the pages in last month's number that Prof. Sampson refers to should have been imperfectly corrected. The principal alterations necessary are these:—On p. 197, line 4 from bottom, for "Lalande" read "Lagrange"; on p. 198, for the sentence in the last line beginning "researches," the following should be substituted, "the analytical theory was revised by M. Souillart"; on p. 199, sixth line from bottom, for "curves" read "years."—EDS.]

OBSERVATORIES.

OXFORD UNIVERSITY.—Reference to our notice of the Report of this Observatory last June will show how the money for printing the Oxford portion of the Astrographic Catalogue (zones +25° to +31°) was provided. The Report of the Savilian Professor dated 1906, April 30, shows that this printing has been begun and that the first volume is nearly completed. It has been decided to publish this work in eight volumes instead of four as originally contemplated, one of the seven zones of declination in each volume, with an eighth volume giving general discussion of the results. Besides this printing and some revision of the plates, which, apart from the Professor's lecturing, formed the staple work of the Observatory, the Report describes the eclipse expedition to Aswan in Upper Egypt, in which Mr. F. A. Bellamy, the First Assistant, took part, and states the results. "Satisfactory photographs of the corona were obtained with the Astrographic object-glass pointed to a 16-inch coelostat, including one through a green colour-screen; and pictures in polarized light (horizontal and vertical) were obtained with two small telescopes pointed to a 12-inch mirror. The polarization was effected by reflection from unsilvered glass surfaces as suggested by Professor Schuster, the method previously adopted of using an Iceland spar prism being abandoned as unsatisfactory owing to some trouble with internal reflections."

The Report makes extensive reference to the lunar work being done by Mr. S. A. Saunder and Mr. Hardcastle with the measuring instruments belonging to the University Observatory now on loan to Mr. Saunder, and to the work being done by other amateur astronomers who have had opportunities granted them for utilizing the resources of the Observatory. Prof. Turner also urges on his Board of Visitors the expediency of making pecuniary arrangements for retaining the valuable services of Mr. H. C. Plummer, M.A., on the staff. It appears that hitherto Mr. Plummer's salary has been supplied in part by the Common University Fund from a grant which was voted for five years—a period which has now nearly expired.

The Meeting of the International Union for Co-operation in Solar Research, held in New College last September at Prof. Turner's instigation, has already been reported in these pages. A resolution passed on that occasion was to the effect that a computing Bureau should be established under the direction of one of the members of the International Committee. The Savilian Professor was asked to organize this Bureau, and to this he assented. A provision of £200 has been received from the William E. Hale fund, and steps to begin operations will now be taken.

STONYHURST.—Drawings of solar spots and faculæ were made on 196 days. The mean disk area of the spots (in units 1/5000 of the visible surface) appears at 6·8 per diem in comparison with 2·54 in 1904 and 0·29 in 1901. The corresponding range of the declination magnet for the three years respectively was 14′·9, 11′·9, and 9′·1. This valuable work of recording the areas of visible sun-spots and the elements of terrestrial magnetism with meteorology appears to be the chief work done at Stonyhurst; but Father Sidgreaves's Report for last year also states, in addition to the above, that the Rowland grating spectrograph has been employed on the larger sun-spots for eye-observations of the red end of the spectrum and for photographs of the green and yellow regions. The equipment of the Observatory has been added to by loans for further research of this kind. The stellar spectrograph has been employed on every available night and 212 exposures have been made.

NOTES.

COMET NOTES.—New elements of the very interesting Comet 1906 *b* (Kopff) have been deduced by M. Ebell from observations extending to April 24 (*Ast. Nach.* 4087):—

T	1905 Oct. 18·662 Berlin M.T.
ω	158° 42′ 11″
☊	342 13 35
i	4 14 32
q	3·328

We may now consider the orbit known with tolerable accuracy, and can say with confidence that the perihelion distance is only exceeded by that of the comet of 1729 (4·1). M. Ebell has computed an ephemeris, both for the past and future, and by the help of this Dr. Wolf has found the comet on a plate taken 1905 Jan. 14, over a year before its discovery; this is quite an unprecedented event, and will enable the determination of the orbit to be made very accurate. The brightness then was 0·4 of that at discovery.

Ephemeris for Berlin Midnight.

	R.A.	Dec.		R.A.	S. Dec.
	h m s	° ′		h m s	° ′
June 5..	11 31 16	0 57 N.	June 29..	11 47 46	0 48
13..	11 35 51	0 26 N.	July 7..	11 52 57	1 30
21..	11 41 2	0 9 S.	15..	11 59 28	2 16

Ast. Nach. 4090 contains a definitive orbit of Comet 1883 I. by Emil Hellebrand. The following is the best parabola, which represents the observations almost as well as the best hyperbola:—

T .... 1883 Feb. 18·9905 Berlin M.T.
ω .... 110° 54′ 35″
☊ .... 278 8 11 } 1883·0.
i 78 3 40
log q ...... 9·880849

A. C. D. C.

Minor Planet Notes.—The following discoveries have been made:—

Letter.	Date.	Discoverer.	Magnitude.	Place.
UB ..	1906 Apr. 16	Wolf.	13·0	Heidelberg.
UC ..	13	”	13·2	”
UD ..	13	”	11·6	”
UE ..	13	”	11·5	”
UF ..	17	”	12·8	”
UH ..	25	Metcalf.	13·0	Taunton, Mass.
UJ ..	25	”	12·5	”

A planet named UG proved to be identical with 149 Medusa.

Planet 554 Peraga is probably identical with CX, discovered in 1896.

Planet TG proves to be still more interesting than was thought last month. Dr. Berberich has deduced elliptical elements from observations made on Feb. 22, Mar. 23, April 22, which indicate a mean distance just greater than that of Jupiter, and an aphelion distance nearly one unit outside Jupiter's orbit, so that the family of asteroids now extends from distance 1·1 (perihelion of Eros) to distance 6 (aphelion of TG). The perturbations of TG by Jupiter should be very interesting owing to the equality of their mean motions. In the present position of the orbits no very close approach occurs. The elements follow:—

Epoch ...... 1906 Feb. 22·5 Berlin M.T.
M .... 48° 57′ 24″
ω .... 120 25 50 } 1906·0.
☊ 315 34 7
i 10 20 53
φ 9 38 43
μ .... 295″·13
log a ...... 0·71999

A. C. D. C.

The Explosion-rings of the Corona.—It will be remembered that a peculiar feature to which the Astronomer Royal called attention in the photographs of the corona taken at Sfax, was the presence of several oval rings or arcs with bright centres, in the neighbourhood of a large prominence, strongly suggestive of explosion. It is interesting to find that three or four such rings, a few minutes of arc above the prominence on the east limb, are seen on the photographs taken during the eclipse of August last by the Hamburg Observatory expedition at Souk-Ahras in Algeria.

Double Stars.—Mr. Aitken is still continuing his survey of the sky with the object of discovering new double stars. The 'Lick Bulletin,' No. 93, bears evidence of his success in an additional 350 pairs, each pair being measured on two or three nights. Of the 350 stars, as many as 276, or 76 per cent., are separated less than 2″. This catalogue brings the total number of pairs discovered by him up to 1250. These are all of similar character to those already published and should furnish many interesting pairs.

Royal Society Conversazione.—The astronomical exhibits at Burlington House on May 9 consisted of photographs taken during the solar eclipse of August last by the Astronomer Royal's expedition, by that under the direction of Sir Norman Lockyer, and by Father Cortie. Six photographs of the Milky Way, taken by Prof. Barnard at Mount Wilson, were also shown, and Prof. Dyson contributed the seismographs mentioned on p. 239. Mr. T. E. Heath exhibited his stereoscopic star-charts.

The Astronomical Observatory of La Plata has been affiliated to the new National Observatory of La Plata, recently inaugurated by the Minister of Public Instruction of the Argentine Republic. It may be remembered that this observatory, of which Prof. Francisco Porro is now the Director, originally proposed to take a zone of the Astrographic Chart, but did not find it practicable to proceed.

At the May Meeting of the Columbia University Trustees, Mr. J. K. Rees, Rutherfurd Professor of Astronomy and head of the Astronomical Department, was made a Professor Emeritus. Mr. Harold Jacoby succeeds Prof. Rees; Dr. Charles Lane Poor will be associated with him as a Professor in the Department, and Dr. S. A. Mitchell is promoted to an Instructorship in Astronomy.

We regret to have to postpone until next month a notice of Dr. Rambaut's recently published Radcliffe Catalogue. We have also received a book called 'New Theories in Astronomy' from Messrs. Spon, and Dr. Herman Kobold's 'Der Bau des Fixsternsystems.'

The President of the Board of Trade has appointed Major P. A. MacMahon, F.R.S., to be Deputy-Warden of the Standards.

The next Meeting of the Royal Astronomical Society will be on Friday, June 8; of the British Astronomical Association on Wednesday, June 27.

From an Oxford Note-Book.

The Astronomer Royal and his Assistants are to be congratulated on the appearance of the quarto volume of 500 pages or so giving full details of the various longitude determinations made in the years 1888, 1892, and 1902. The first date has receded from us at a surprising rate, and seems to murmur something about delay in publication; but there is a good answer ready. The same problem runs through the observations in all three years, and there is thus a distinct gain in publishing them all together. The problem is that of obtaining accordant values for the same longitude when two pairs of observers determine it independently but simultaneously. When preparations for determining the longitude Paris—Greenwich were being made in 1888 it was not imagined that there would be any great difficulty in securing this accordance, and when the two French observers found a result $0^{s}\cdot21$ in excess of that found by the two English observers some consternation was felt. It is possible to explain away this particular difference, as the curious may learn by referring to the *Monthly Notices*, vol. li.; but on repeating the determination in 1892, the French observers found a result even more in excess of the English, viz., by $0^{s}\cdot26$. A third attempt in 1902 with different observers and different arrangements reduced the excess to $0^{s}\cdot04$, but it remains in the same direction, and the three considerable pieces of work taken together seem to bear the interpretation that two French observers determining the longitude Paris—Greenwich will find a result systematically in excess of two English observers.

This result has a paradoxical appearance, for, of course, great care was taken to eliminate "personal equation" by the interchange of observers. It is not, however, impossible to devise a fair explanation for it as it stands, though other evidence does not confirm the suggestion. But may I first relate a little incident leading up to the explanation? I saw something of the first two Paris—Greenwich longitudes, and remember well an occasion when one of the observers, dining, as was the custom, in company with his French colleague, ordered a pint of white wine as a change from the usual red. He became aware that his colleague was regarding him with some anxiety, which ultimately found expression in the remark: "Ça changera l'équation personnelle, vous savez!"

Now this particular contention may have been a little strained; but does it not seem possible that when an observer leaves his own home for foreign lands, and has his usual habits more or less upset, his personal equation may be slightly altered, and that this alteration may always be in the same direction? Not necessarily the same for all observers, but in the same direction. We have an instance of a similar kind in the case of "magnitude equation." Observers differ in the amount, but, as a rule, they all observe faint stars late rather than early. If, then, we may make the assumption that disturbance of regular habits increases the average personal equation by a quantity x, then it is easy to see that two English observers, one of whom is always observing in Paris, will obtain a longitude $L - x$, and two French observers, one always at Greenwich, will obtain $L + x$, so that the French result will be systematically in excess of the English by $2x$. The values of x may vary, as above stated, for different observers and at different times, according to the amount of "change of habit": the mean value of x indicated by the above three determinations is about $0^s{\cdot}08$. This seemed a possible solution until it met the critical eye of Mr. Hollis, who pointed to the simultaneous determinations Paris—Berlin and Paris—Bonn, mentioned on p. 2 of the volume in question, which go the other way. The French observers are indeed again in excess of their colleagues by $0^s{\cdot}13$ and $0^s{\cdot}03$ in the two cases: but these are applicable to East longitudes instead of West, and their signification is thus precisely opposite. To fit these facts we must suppose that the personal equation of an observer is always decreased by travelling eastwards; and even then we seem to find that the change is greater when he only goes a little way east than when he takes a longer journey.

THE price of 'Nature Knowledge in Modern Poetry,' by A. Mackie (Longmans), being only half-a-crown, it seemed desirable to invest that sum for a glance at the contents. The modern poets dealt with are Tennyson, Wordsworth, Matthew Arnold, and James Russell Lowell, and the main object of the little work seems to be to draw attention to the superiority of the first named as an observer of Nature. He is considered in four chapters as botanist, entomologist, ornithologist, and geologist. The conciseness of this classification makes it necessary to find a place for oddments in curious pigeon-holes. Thus we read of

> Unnumbered and enormous polypi

in the chapter on entomology. We cannot therefore complain that astronomy is made a subsection of geology, though we may feel as though we were standing on our heads. We reach our special topic through the references to the Nebular Hypothesis:—

> This world was once a fluid haze of light,
> Till toward the centre set the starry tides,
> And eddied into Suns that, wheeling, cast
> The planets.

The reference for this extract is given as 'The Princess,' though Mr. Mackie often omits the references altogether in a tantalizing manner. He then proceeds (p. 50):—

> Spectrum Analysis is thus alluded to in 'In Memoriam,' xxi.:
>
> When Science reaches forth her arms
> To feel from world to world and charms
> Her secret from the latest Moon.
>
> This brings us to Astronomy.

So it does, no doubt, if we are not even already arrived; and probably others besides myself will be puzzled to detect the allusion to spectrum analysis. Is not the reference rather to the discovery of Neptune from the study of Uranus? The phrase "To feel from world to world" reminds us of Sir John Herschel's words, "Its movements have been felt trembling along the far-reaching line of our analysis"; and perhaps this actual phrase may have been in some way responsible for Mr. Mackie's mistake—he may, in fact, have misunderstood what was meant by analysis. 'In Memoriam' was published in 1850, only a few years after the discovery of Neptune; and if we remember that the discovery of the satellite of Neptune followed that of the planet within a few weeks, and was itself followed in 1848 by the finding of Hyperion, we can almost date the lines quoted as having been written soon after this last sensation in 1848. It is not the only, or the most striking, instance of a poem of Tennyson being dated by internal astronomical evidence in this way. In vol. xvii. of this Magazine, p. 93 (February 1894), it is shown how the lines in 'Maud,'

> And pointed to Mars
> As he glowed like a ruddy shield on the Lion's breast,

must refer to the period in April and May 1854 when Mars was "stationary" in Leo. These lines are not quoted by Mr. Mackie, though he has noticed those which occur almost immediately preceding, fixing the time of year when

> The Charioteer
> And starry Gemini hang like glorious crowns
> Over Orion's grave low down in the West.

Altogether, Mr. Mackie does not seem to have made the most of his opportunities so far as astronomy is concerned; and if his other science is no better, the expenditure of even half-a-crown is not to be recklessly incurred.

But on the other hand, the modest sum of sixpence (or was it 4½*d.*?) invested in *Pearson's Magazine* (vol. xxi. pp. 402–409) told me all about "The Professor's Mistake: how it brought him happiness. The story of a Miscalculation." By Dorothy Hilton. The story opens as follows:—

> The Professor laid aside a mass of papers, notes, observations, and other astronomical data which he had been revising, and sat himself down to think. It was a big situation that the poor man had to face—nothing less than the certainty that in three months the career of this wicked old world was to come to an abrupt termination. He had known this for some time, but, to make assurance doubly sure, he had spent the last week in working out his calculations afresh. The result was the same, and he had now accepted it as an incontrovertible fact which needed no further discussion.
>
> This was June, and in the early part of August the end was to come. There was just a possibility that the doom might be deferred until the latter part of the month, but the 31st was the cancelling date, so to speak, of the Professor's calculations; after that there was nothing to calculate.
>
> He faced the situation quite calmly; he could even smile in a superior way at the folly of the great astronomers who were still prating idly of the wonderful meteoric showers to be expected from that very combination in the Solar System which the Professor was certain would produce such a different result. Yet he had no desire to flaunt his discovery in the face of the scientific world. He did not consider that the pleasure of saying "I told you so," at the moment when the Earth was reeling into nothingness, would compensate him for figuring during the last three months of history as a crack-brained croaker.

The calculations were apparently wrong, as he found when the "hot days of August wore away, and this old Earth of ours still spun on in her orbit," and especially when "the morning of the 31st dawned serene and smiling." He had placed himself in an "appalling situation," having gambled, so to speak, on the event by marrying a wife, who had been led to consider him a man of means because he had felt justified in spending capital. He explains his motives when the clouds have lifted by stating that "he was not certain that he could make a woman happy, but he thought he couldn't make her *very* miserable in three months." Miss Hilton seems clear that the capacities of Professors are strictly limited in a number of directions. One wonders whether she has studied from the life?

THE following three answers were kindly sent to me by an examiner in South Africa:—

> The vernal equinox is that time when the Sun is perpendicular to the equator.
>
> Right Ascension is the time for a body when it just creeps above the horizon.
>
> Declination of a body is when its axis is turned away from perpendicularity to its plane.

THE Bishop of London, speaking at Hampstead, is credited by the *Church Times* for April 6 with the following utterances respecting the whole duty of a star:—

> The stars on a dark night bear witness to the Sun, and so by faithful witnesses in every walk of life, &c. &c.
>
> What an awful thing to end our lives as dead failures, wandering stars who ought to have been reflecting the Sun, lost in the mists of darkness for ever.

372.]   JULY, 1906.   [*Price* 1

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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VOL. XXIX. JULY, 1906. No. 372.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1906 June 8.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder. Sixty-seven presents have been received since the last Meeting. Amongst them will be found Sir William Huggins's book, 'The Royal Society,' which has been presented to us by the Author, and a volume of the Report on the Geodetic Survey of Rhodesia, presented by Sir David Gill. We have also received from the Royal Observatory, Greenwich, 18 enlargements of Astrographic Chart plates in continuation of the series now being made, and ten enlargements from the San Fernando Observatory.

A vote of thanks was accorded to the donors, and the President invited Mr. Cowell to begin the proceedings by making some remarks on the subject of his researches on Ancient Eclipses.

*Mr. Cowell**. I am about to give an account of two criticisms of my papers on Ancient Eclipses, and of a reply of my own to these papers. The longer of the two papers is by Mr. Nevill; and Mr. Nevill's views on the motion of the Moon deserve the very closest attention. In the past it was he who attributed Prof. Newcomb's empirical term to the action of Jupiter, thereby creating a new branch of the lunar theory, and laying the foundation of a subject that M. Radau has worked out at some length, and which happens to form at the present time the subject proposed for the Adams Prize Essay at Cambridge. And, moreover, Mr. Nevill was the first to remove an error in the methods of analysis of modern observations of the Moon. Airy, Hansen, and Newcomb all introduced errors into their determination of the eccentricity of the Moon's orbit, because they used wrong coefficients of sin ($g-D$). Mr. Nevill was the first to appreciate that an error of this sort was of importance.

---

* [This report is not *verbatim*. This succinct account of a very technical subject has been kindly re-written for us by Mr. Cowell, and embodies the whole of the facts he laid before the Society.—EDS.]

I originally showed that certain corrections were the logical deduction from the interpretation which I placed upon five solar eclipses, and I also stated that the eclipse of Agathocles fitted in with these corrections. Subsequently I found that the lunar eclipses of the 'Almagest' fitted on as well. My position is therefore that I cannot doubt that these corrections are appropriate to ancient eclipses; that is to say, the distance of the Sun from the node is subject to a secular acceleration of an amount not suspected by theory. As there seem to be the gravest objections to altering the theoretical motion of the node, I therefore inferred a secular acceleration of the Sun. Both Mr. Nevill and Prof. Newcomb consider that the modern observations disprove a secular acceleration of the Sun. As Prof. Newcomb also holds that the theoretical secular acceleration of the node is above suspicion, it follows that he considers that my conclusions must of necessity be erroneous; and of course it is easy to say in the case of each solar eclipse that in some detail or other it is open to misinterpretation, and that the lunar eclipses are not of sufficient accuracy. Prof. Newcomb allows nothing whatever for coincidence. Granting that lunar eclipses are not very accurate observations, it is still surprising that in nine cases out of ten their recorded magnitudes should be larger or smaller as required by my hypothesis than the magnitudes as given by the present tables. Granting that, in the case of the eclipse of −430, for example, Thucydides might not have been at Athens, still it is natural to suppose that he was there, in the absence of evidence to the contrary, and it is remarkable that several eclipses should support the same hypothesis.

I cannot agree that modern observations are at all decisive. It is true that they do not indicate a secular acceleration, but they are liable to large errors of personality, and, moreover, a secular acceleration might easily be concealed by a long-period term. As we clearly do not know all the long-period terms of the Moon, we have no right to deny the possibility of their existence in the case of the Sun. On the other hand, I feel the great weakness of my position, when I have to suggest that something which others do not admit is concealed by a second something which is purely hypothetical; but I do maintain that ultimately the Moon's motion (or the Sun's) will be found to fall into line with these eclipses. When more than one alternative presents itself, as in the present instance, I can only speculate.

Now Mr. Nevill, although absolutely refusing to accept a secular acceleration for the Sun, is content to examine the evidence for alternative of a secular acceleration of the node. He does not consider the lunar eclipses at all, nor does he give reasons for not doing so; but he puts forward in all twenty-eight solar eclipses as against my original five. His own verdict is, I think, "Not proven." I think I may reasonably infer that it is not "Disproved" when he writes: "the preceding examination . . . . strengthens Mr. Cowell's view, for. . . .his data serve to represent other eclipses. . . .in particular. . . .the eclipse of Larissa, the most critically important of them all." Mr. Nevill is certainly a most courteous critic.

Now, in selecting my original five eclipses, my criterion was contemporary evidence. According to Mr. Nevill's calculations, supplemented by the remark I have found in *M. N.* xxiv. p. 42, that of Mr. Nevill's Chinese eclipses Nos. XIII. to XXIV., only the eclipses of −600 and −708 are described as total, the case against my hypothesis must be based on the eclipse of Susa, where the Sun was troubled for three days, or on the eclipse of Ennius, described in vague language by Cicero 350 years later. The eclipses of Pelopidas and of −381 (Mr. Nevill's XXVIII.) are not evidence against my hypothesis in particular, for no admissible corrections will make them total. In fact I have every reason to be satisfied with the way in which my hypothesis stands the test of Mr. Nevill's calculations. I must admit that he has, by historical reasoning, cut away one of my original five eclipses—the eclipse of Archilochus—and that I ought not to have used it. We may in time be able to say that there are astronomical reasons for supposing that Archilochus was at Thasos in −647, but I now fully agree with Mr. Nevill that there is no sufficient historical proof of the fact.

Mr. W. T. Lynn. It may be remembered that Thucydides, as an Athenian, had large possessions in Thrace, and went there frequently, and on one of these visits he may have seen the eclipse.

Mr. Cowell. On the other hand, if he had seen it from Thrace and had written a book saying the stars appeared, and shown that book to his Athenian friends, they would probably have told him that they did not see the stars appear. I admit there is something to be said against a single piece of evidence, but in this case we have so many pieces of evidence pointing in the same direction.

Prof. Turner. We are all greatly interested in the notes made by Mr. Cowell on Mr. Nevill's paper, and especially in the fact brought out so clearly that Mr. Nevill must have such a large amount of excellent theory worked out. This he has, indeed, told us before now; but we have it put into actual evidence by the present paper and the one published a month or so ago criticizing Mr. Cowell's lunar work in great detail, and we can only emphasize our regret that all this work should be delayed in publication so long. Perhaps others might have got suggestions from it if only it had been published. I understand that there are difficulties with the Natal Government, but I cannot help thinking that the time has come when, if the Natal Government cannot publish the work, some other body should be approached with a view to getting it published. I also feel that we must admire the courage with which Mr. Cowell goes forward in the line of thought that he introduced to us in spite of a good deal of, not exactly discouragement, but added weight which he seems to have to carry as he goes along. The historical criticism of Mr. Nevill is not by any means entirely destructive, as Mr. Nevill points out carefully in his paper, but at the same time it is not strongly confirmative of Mr. Cowell's conclusions. If I understand him rightly, other hypotheses will give almost equally good accordance between the old eclipses and modern observation. In one sense that is satisfactory, because it shows that we need not put these eclipses aside altogether, as has been suggested, but that there is at least one way, if not more, of reconciling them with some modified theory. But then, of course, we have been introduced to-day to the fact that modern observations of the Sun do not quite bear out this supposition of Mr. Cowell, unless we make another supposition. Now the value of his suggestion that the terms in the Sun and in the Moon really arise from the same cause depends greatly on the fact whether the phase of the two agrees. I do not quite gather whether Mr. Cowell has examined that point. [*Mr. Cowell* said "Yes."] If the phase agrees, of course the testimony is enhanced enormously; and as I understand that is so, it is a most interesting addition to the theory. The only other matter I might refer to, as I have raised the question, is the fact that the lengthening of the day which Mr. Cowell has adopted as an explanation of secular acceleration would introduce terms into other planets which have not been confirmed as yet. Perhaps Mr. Cowell may find confirmation of them: in the case of Mercury, for instance, there should be secular acceleration due to change in the length of the day; but this acceleration has not yet been identified—it may be obscured by a long-period term. These difficulties might daunt a less courageous man, but Mr. Cowell, having convinced himself that he is on the right path, has set himself to clear away all these difficulties one by one, and we cannot but admire this courage.

The President. I think we all appreciate the very heavy character of the work Mr. Cowell has undertaken. I will ask you to return thanks to him for the way he has presented the subject to us to-day.

The Astronomer Royal (presenting the paper on "Errors of Jupiter's Tabular Place from Photographs"). This is a pendant to the paper read at the last Meeting on the results of micrometer measures of the positions of Jupiter's Satellites VI. and VII., as shown on photographs. It may be remembered that these photographs were taken between November 3 and February 15 last, so that they included the opposition of Jupiter, and for purposes of reduction supplementary photographs of Jupiter and the field around were taken with the 13-inch Astrographic telescope, and from these the errors of the tabular places of Jupiter have been determined. The measures have been reduced by the ordinary method, small corrections, such as division-errors of réseau, having been applied, but they do not represent the highest accuracy, because no occulting-shutter was available in this case, and the image of Jupiter was much over-exposed. I am sorry I have not a slide to show the comparison between the errors found from the photographs and those from observations with the transit-circle and with the altazimuth, but from the figures I have written on the board it will be seen that there is good agreement between the photographs and the transit-circle, especially in declination, but the comparison with the altazimuth does not come out so well. This is of some interest as it provides in some degree a check on the value of the R—D correction that has been adopted in the reduction of the transit-circle observations. I think it will be seen when the paper is printed that the probable error of the results from the photographs is smaller than that of those from the meridian instruments.

Prof. Turner asked whether the R—D was really checked. He supposed any photograph of the place of Jupiter was referred to stars which had been observed at Greenwich and corrected for R—D, so that photographic observations would naturally agree with meridian places.

The Astronomer Royal. I did not mean that the accordance was entirely due to the R—D correction. My remark referred rather to the comparison between the two instruments—the transit-circle and altazimuth. It will be seen that the results from these differ considerably, and this may be explained by the application of the R—D correction.

Dr. Rambaut. So far as I have been able to follow the paper, I cannot see what the evidence is which leads the Astronomer Royal to suppose this discrepancy is due to R—D more than some other sort of error. It is evident there is a small disagreement between the results obtained with the altazimuth and the transit-circle, but I must confess I fail to see why it should be attributed to R—D.

The Astronomer Royal. I do not attribute it to that. There is discordance in right ascension also, but as to the declinations, if we adopted different values of the R—D correction we should get a different result. It seems to me the point is that the adopted value of the R—D correction is probably slightly erroneous for both of them. It is difficult to settle what is the proper value to adopt, because it is quite possible that the observed discordance between direct and reflection observations is due to the reflection observation only, and not to both. We assume it is equally distributed between the two, and as a check on this our planetary observations may come in useful.

The President. It is interesting to have these results, and I hope they will accumulate at Greenwich. We have to thank the Astronomer Royal.

Mr. Newall (introducing, at the President's request, the subject of last year's Eclipse). It had seemed to me right not to present a further note on eclipse phenomena before having done more by way of successful measurement and study of the photographic records obtained at the last eclipse, and so had earned the title to speak, and I had looked forward to carrying out that further study during the summer. But the kind interest of the President has raised another question, viz., whether we observers have earned the title to remain silent. Your interest is an encouragement, and I gladly fall in with your suggestion that a gathering together of the main results may be of interest. The observations which I gather together in my note relate chiefly to the polarization phenomena. Even if one deals with the observations alone and leaves out of consideration the interpretation of them, the subject may be described, with a judicious mixture of metaphor, as bristling with pitfalls. I find my own observations are in conflict with those of M. Salet in regard to so simple a matter as the position of the plane of polarization of the light scattered by the Earth's atmosphere. We observed at stations separated by only 30 miles. He arrived at the conclusion that the plane of polarization was vertical, and I have conclusive evidence, along three distinct lines, that the plane was nearly horizontal. Again, Dr. Gilbert and I, observing within a mile of each other, are at variance about the evidence as to the existence of signs of polarization in the innermost corona.

The speaker then went on to refer to the points summarized in his paper, and dwelt upon the fact that his photographs of polarized spectra showed indications of marked polarization in the inner corona, but no Fraunhofer lines. He referred to the difficulties met with in the attempt to explain by means of radiation-pressure the repulsion of dust along streamers that show most polarization. But while adding that he regarded it as unprofitable to try and solve a quantitative problem by qualitative considerations, he said he hoped before long to have definite quantitative results to bring before the Society.

The Astronomer Royal (being called upon by the President). I have nothing new to bring before the Society. I think I have already shown lantern-slides of our results, and at the last Meeting I showed prints copied in the camera from the negatives as a suggested method for reproduction and distribution. The next step will be to get a drawing made giving the combined result of the photographs. As some present may not have seen them, it may be worth while to show the photographs again.

(The photographs of the eclipse of August 1905 obtained at Sfax were shown on the screen and explained by the Astronomer Royal.)

Prof. Turner. I am afraid that, like Mr. Newall, I have got a good many measurements still to make. But the preliminary measurements which one has been able to make brought out two important facts, and for another purpose I was led to follow hese two quantitatively, and have been a good deal interested to see how much they limit the field of speculation as to the nature of the corona. I will, then, just make one or two remarks briefly as to these calculations. The facts I have started from were simple. In the first place, there is strong polarization in the corona. Mr. Newall has brought out that fact, and the pictures I will show you illustrate it also. There is no doubt about the light of the corona being strongly polarized from the measurements made. I feel one may say one-third or two-thirds of the light shows polarization, but accurate measurements are still to be made. Another set of measurements I have made on photographs has given the law of degradation of the brightness of the corona, as we proceed outwards from the limb. Mr. Newall referred to the approximate law: the light falls off as the sixth power of the distance from the Sun's centre. And it is that quantitative statement which I take as the starting-point in the small calculations I made, assuming, in the first instance, that the light is strongly polarized, and therefore due to the existence of small particles comparable with a wavelength of the light. This law of brightness ought to tell us in the first instance the law of distribution of these particles, *i. e.* their density. Calculations so far were made by Prof. Schuster in an important paper of his in volume xl. of the 'Monthly Notices,' which is the classical paper on this subject; and the first deduction from his calculation is: Given the law of brightness to deduce the density of the particles. It is obvious that if the corona is orb-shaped and we look in any direction, we get light reflected from all these particles at different angles, and we have to make this calculation: Given that the density is so much at different distances, how much light do we get in this particular direction from the Sun's limb? The way Prof. Schuster performed the calculation was to assume different distribution of density. From his figures (though not explicitly given in his paper) it is easy to calculate that the power of the distance representing brightness exceeds the power of the distance representing density by about 1·5. Whatever be the law of density, the brightness seems to give 1·5 greater as power. Now our observations of the law of brightness give 6 for the index; so that the index for density of distribution is 4·5.

The next point was this: I examined what physical idea of the corona would fit in with that law of density. Suppose, for instance, that the light-pressure or electrical repulsion were actually strong enough to overcome gravity altogether, and the particles were continually streaming *outwards* from the Sun, it would be easy to see what would be the law of distribution of density. In the first place, if they start out from near the Sun, and ultimately occupy a much larger space, we know they would be more thinly scattered over a larger sphere, and we know the mathematical expression would be the square of the distance. Also they would be thinned out because they are going quicker across the same distance when further out; under a repelling force they would be moving continually quicker, and so we get another increase. But that increase is not large; if the force is continually repelling outwards it cannot be greater than half, so that the density of particles, if they are continually streaming upwards, would be only 2·5, which is very different from 4·5, so that our measurement seems to show conclusively that particles cannot be continually travelling outwards. One may, indeed, say "Certainly not *all* particles, but may not *many* of them continually be travelling outwards?" The answer is that, if some are travelling outwards, we must look for a power even higher than 4·5 in the case of the rest, because we have diluted the fact. So that we have to look for a physical distribution which will give 4·5 at least as the index for density. Now we might take the other extreme. Suppose they were continually falling into the Sun from space. Then we find that we do worse still, so I need not dwell on it. So that we are led to suppose they are projected outwards and fall back. Now there are two variables for things projected out from the Sun, the actual amount of velocity and the direction. We may as well take these separately and see the effect. Let us suppose first that the velocity is constant in magnitude and merely varies in direction, so that there are some particles going straight up and falling back and some following parabolic, or elliptic, or paths of that kind. If the magnitude of the velocity be just such as would send a particle, if projected horizontally, round the Sun as a close satellite, it can be seen that whereas there are particles at other distances for an infinitesimal time, there are particles at the distance of the radius for a finite time; so that we get infinite density at the Sun's surface. Well, that would do for the photosphere perhaps, but the supposition could only give us corona for one radius from the limb. And if we increase the velocity of projection we are met by the fact that we get a certain distance from the Sun at which there is infinite density. In fact, varying the direction of projection will not help in forming an idea of the constitution of the corona, so that we are driven back upon variation in magnitude of velocity of projection. It is the same, however, whether we assume velocity varies or the resultant force varies; and since the particles are small, comparable with a wave-length of light, the repulsion of light will be nearly equal to gravity, and the resultant attraction will be small, and if these particles vary in size the difference between the two will vary in size. The fact that the particles are small seems to supply an element we want, viz., large variation in the attractive force of the Sun. My conclusion is that, for the present, we may at any rate accept the hypothesis of particles about the size of a few wave-lengths of light, but still not so small as to be repelled.

Prof. Turner then showed some slides of the corona and its polarization, and in describing them said:—One fact I must put forward apologetically as a correction to something already said. When giving an account of the damage done to Mr. Reynolds's photographs, I said we had no experience of the kind ourselves. Mr. Bellamy has drawn my attention to the fact that there is one plate rather badly marked by the woodwork of the slide. I think only one slide, but I had forgotten it at the time. At any rate, here is the picture for you to see.

Mr. C. T. Whitmell. On looking at plane polarized light, I easily see Haidinger's Brushes, a blue brush crossed by a yellow one, and can thus, without any apparatus, recognize that the light is polarized, and also determine the plane of polarization, which is indicated by the yellow brush. The phenomenon is perhaps known to physiologists better than to astronomers.

Prof. Fowler. I gather that one of Mr. Newall's difficulties is to account for so much polarized light in the inner corona which does not exhibit the Fraunhofer lines in the spectrum. Before admitting this as a real difficulty, however, I should like to be assured that nothing has been overlooked. I take it that there is no question as to the presence of Fraunhofer lines in the spectrum of the outer corona, and that it is only in the inner corona that they have not been observed. Now we know from previous work that the inner corona gives a continuous spectrum in addition to its bright lines, and I am not yet quite convinced that this is not sufficient to swamp any dark lines which may be present on account of reflected sunlight. Some years ago, Prof. Wood described a very interesting experiment in this connection; he placed an ordinary gas-flame in a beam of sunlight and found that while the Fraunhofer lines were visible in the spectrum with strong sunlight, there was a point in the intensity of the solar beam below which the dark lines could not be seen with an ordinary spectroscope. With the aid of a Nicol, however, the relative intensities of the spectra from the two sources of light could be changed and the dark lines rendered visible. I imagine that Mr. Newall's experiment of producing two spectra of the corona with a double-image prism corresponds with this, but I should like to ask if he has made a similar observation with an ordinary image of the Sun on the slit, and if any of the difference in the intensities of the two spectra may be due to polarizing effects in the optical train. I should also like to elicit Mr. Newall's opinion as to Prof. Wood's suggestion that the apparent absence of dark lines from the inner corona may be due to their broadening, arising from varying motion in the line of sight of the particles reflecting the solar light.

Mr. Newall. I should not have dreamt of making my statement as to the difference between the polarized components in the spectra unless I had known what effect the polarization due to the instrument that I was using would have on the photograph. I have taken account of such differences as would be produced, simply by having done what Prof. Fowler suggests, namely, by having taken photographs of the Sun and the sky as unpolarized sources, and comparing them with the coronal photographs through the double-image spectrograph in exactly the same adjustment.

With respect to the other point—the interesting experiment of Mr. Wood—I have made experiments also dealing with that point, and I may say the experiment made in the spring of this year shows that dark lines can be detected photographically when a mixture of one part skylight with three parts of continuous light from a gas-flame is passed into the spectroscope. Hence one would expect to find the Fraunhofer lines if the polarized light amounted to only one-seventh of the whole corona light *.

Prof. Fowler. I scarcely imagined that Mr. Newall could have omitted to make the check-experiment to which I referred, but I am glad to have a definite statement on this point, as well as the additional information which he has given us.

Father Cortie. Although the theory which would explain the forms of the coronal streamers as due to the pressure of light is a very fascinating one, yet it seems to me that many of the phenomena observed can be accounted for by the ejective forces residing in the Sun. Observations of prominences in the spectroscope show that these ejective forces acting on the materials in the neighbourhood of the Sun's photosphere are extremely vigorous and active. Besides, we know that the general form of the solar corona in different years is intimately connected with the number and extent of spots and prominences visible on the Sun. Recently I have been endeavouring to find, beyond this general well-known connection of spots and coronal form, evidence of connection between particular forms in the corona and centres of sun-spot disturbance. On the picture of the corona of 1905 August 30, now on the screen, it will be noticed that the eastern side of the corona shows greater signs of disturbance than that on the western side of the Sun. In particular, there is a group of plumes which, although they radiate from the apparent limb of the Sun at various angles, yet converge to a point or area situated about 20° to the south of the Sun's equator. Of course these plumes which existed in three dimensions, and on the pictures are projected into two, may radiate either from an area on the visible side of the Sun or on that turned away from us. Having determined the heliographic positions of the apparent radiation-centre of these plumes, the spots actually visible on the Sun on the day of eclipse, as also the centres of greater disturbance during the year, were next plotted on a diagram in their true heliographic positions. There were four spots visible on the Sun on the day of eclipse, and the greatest of these was situated in South latitude 20°, and removed 41° from the limb. The same area had been the seat of a great July spot visible to the naked eye. On the other hand, the area disturbed by the great February spot, the greatest seen for the last thirty years, was in the same approximate latitude ($-16°$) and removed 31° from the limb on the invisible hemisphere of the Sun. Moreover, of the four spots visible on the day of the eclipse, and the six other centres of disturbance between February and the end of October plotted on the diagram, no less than seven of them were near the east limb on the day of eclipse, one was nearly on the central meridian, another about 20° west of the central meridian on the invisible side of the Sun, and the last, the area subsequently occupied by the very large October spot, but which, probably, at the end of August had not yet become an active centre, removed some 40° from the western limb. The plumes, therefore, seen on the eastern limb in the corona correspond to the side of the Sun which was the more active, as far as spot-centres are concerned, on the day of the eclipse, and had been more active in the month preceding the eclipse. Their radiant-point, too, corresponds to either the great February spot of 1905, or, if on the visible side of the Sun, to the position occupied by the biggest spot on the day of the eclipse, which, too, was near to the position of a centre of great activity a month previous to the eclipse.

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* *Note.*—One must be careful that in devising mechanism to explain the absence of Fraunhofer lines in the innermost corona one does not endanger the explanation of such lines in direct sunlight.

Mr. W. B. Blaikie explained, by means of a mechanical slide projected on the screen, an ingenious device called "a mechanical slide-rule." This consisted of two circular disks, the upper one being transparent, having one centre about which the upper could turn. On these disks were traced accurate ellipses, to represent projections of great circles on the sphere, and by setting the upper circle in appropriate positions it was easy to obtain the approximate solution of the spherical triangles in certain astronomical problems.

The following papers were announced and partly read:—

Lewis Boss. "The New Reduction of the Meridian Observations of Groombridge."

Simon Newcomb. "On Mr. Cowell's Discussion of Ancient Eclipses of the Sun."

R. Crawford. "The Physical Condition of Mars."

Royal Observatory, Greenwich. "Results of Micrometer Measures of Double Stars made with the 28-inch Refractor in the Year 1905."

Royal Observatory, Greenwich. "Errors of the Tabular Places of Jupiter from Photographs taken with the 13-inch Astrographic Refractor."

A. A. Rambaut. "A Simple Method of obtaining an Approximate Solution of Kepler's Problem."

A. R. Hinks. "Solar Parallax Papers: No. 4. The Magnitude Equation in Meridian-Circle Right Ascensions of the 'Étoiles de Repère."

Mr. Newall. "Notes on Polarization Phenomena in the Solar Corona."

P. H. Cowell. "On Ancient Eclipses."

The following Foreign Astronomer was balloted for and elected an Associate of the Society:—

Prof. Julius Franz, Observatory, Breslau, Germany.

The following gentlemen were elected Fellows of the Society:—

John de Fenton, Ph.B., Seymour Avenue, Parktown, P.O. Box 1050, Johannesburg, South Africa.
John Grigg, The Observatory, Thames, New Zealand.
Robt. Leetham Jones, M.A., 3 King's Bench Walk, Temple, E.C.

The following Candidate was proposed for election as a Fellow of the Society:—

Alexander Davidson Fleming, Artillery Mansions, 78 Victoria Street, S.W. (proposed by Rev. D. Fleming).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1906 May 23.

A. C. D. Crommelin, B.A., *President*, in the Chair.

Secretaries: J. A. Hardcastle.
J. G. Petrie.

The Minutes of the previous Meeting were read and confirmed.

Among the presents received, the President drew particular attention to a pamphlet by Dr. Downing on "The Total Solar Eclipse of January 3rd, 1908," which would be visible from two coral islands in the Pacific. As the Association had been represented at all recent total solar eclipses, he (the President) hoped it would be represented at this, although the journey would be a long one.

The President said the Council were sorry that the report of the late eclipse had been so long delayed owing to various causes—chiefly the lamented illness of Mr. Thwaites,—but it would be sent out with the next number of the 'Journal.' A copy would be sent free to every Member of the Association, but it was hoped that a large number of Members would buy an extra copy so as to help defray the cost of the extra number. For extra copies Members would be charged 1*s*. each, the price to non-Members being 1*s*. 6*d*.

Mr. W. T. Lynn read a paper on "The Expected Return of Holmes's Periodical Comet." At its first recorded appearance in 1902 it was discovered independently by three astronomers—Mr. Holmes, on November 6; Dr. Anderson, at Edinburgh, on the 8th; and by Mr. J. E. Davidson, at Mackay, Queensland, on the 9th. This comet was remarkable for its small eccentricity, which amounted to no more than 0·41. The perihelion distance of the comet, according to Dr. Zwiers's ephemeris, was 2·123, the aphelion distance, 5·096, being very nearly equal to the distance of the planet Jupiter. There appeared to be a good probability of its being seen, but only with powerful telescopes, in the forthcoming autumn, the most favourable time being the moonless nights in the middle of October.

The President said that this comet, discovered by Mr. Holmes 14 years ago, was a very interesting member of our system, and thanks were due to Mr. Lynn for having prepared the paper upon it which he had just read. He was afraid that unless there was some unexpected outburst of brightness, the comet was hardly likely to be seen in this country next autumn.

The Rev. T. E. R. Phillips gave an abstract of the Jupiter Memoirs for 1903–4.

The President said he was very glad to see that the Jupiter Section was so active. There was a wide field for careful and sustained observation. The observations of a satellite through the limb of the planet were particularly interesting. The late Mr. R. A. Proctor laid great stress upon similar observations as showing that the outer part of the disc of the planet must be of a cloudy or gaseous nature, and of course that was the only explanation of seeing a satellite through an enormous thickness of planetary matter.

Mr. H. P. Hollis gave a short address on the figure of the Earth, with reference specially to an hypothesis as to the cause of earthquakes which had recently been referred to in an article in the *Times.* He sketched the history of the determination of the Earth's figure, pointing out that there were reasons for thinking that the Earth's equator might not be a true circle but an ellipse of small ellipticity, and that the longer axis of the ellipse passed through a longitude not very far from the meridian of Greenwich. M. Poincaré and Sir George Darwin had shown that under certain conditions of elasticity and speed of rotation a planet might be of a form other than spheroidal. Mr. Jeans had been led to conclude that in the case of the Earth the spheroidal shape was one of stability, but that at some time the Earth had been pear-shaped, and that it might still bear traces of that form. Mr. Jeans had suggested that the Earth might be arriving at a spheroidal form from an apioidal by a series of ruptures about the equator of the pear. The British Isles occupied the centre of a hemisphere which is practically all land, the other hemisphere being practically all water, and it was likely, for this reason, that England or some point near was at the blunt end of the pear. Although it was not possible to entirely reconcile the results of geodesy with those of mathematics and the suggestions of geologists, still the three sciences all pointed to the existence of an axis of symmetry other than the axis of rotation.

Mr. Hardcastle showed on the screen some copies of diagrams taken from a lecture by Prof. Sollas, illustrating Prof. Jeans's pear-shaped Earth, and the way in which such an Earth would "crumple" under the strains produced by gravitation.

The President said the subject introduced by Mr. Hollis was exceedingly interesting. It was natural for astronomers to wish to know the exact shape of the world they lived on, and it had a bearing on the distance of the Moon, because they could find the distance of the Moon only as compared with the size of our own planet.

Mr. Seabroke said that since Mr. Newbegin produced his slide showing the solar prominences of the year 1905 aggregated at their several position-angles, he had been curious to ascertain how far his observations agreed with those made by himself in 1873, three periods of 11 years ago, and he had, therefore, combined those for the first half of 1873, and showed the slide on the screen, pointing out that the positions of maxima corresponded on the whole with the results obtained by Mr. Newbegin.

The President said it was very satisfactory to see the close agreement between Mr. Seabroke's diagrams of prominences and that made by Mr. Newbegin. It showed that they seemed to follow the sun-spot curve very closely indeed.

Mr. C. Thwaites showed lantern-slides of three photographs of sun-spots enlarged in the telescope by means of an ordinary eye-piece, so that the image of the Sun's whole disc would be about 15¾ in. in diameter. The picture thrown on the sheet was again magnified by the lantern about 34 times.

The President said that, considering the immense enlargement Mr. Thwaites's photographs had undergone, their definition was wonderfully creditable. He was glad to hear that Mr. Thwaites was going on with this series.

Mr. Petrie read a paper by *Col. E. E. Markwick* on "A Study of β Lyræ," being an interim Report of the Variable Star Section.

The President said they had to congratulate themselves upon having such an energetic Director of the Variable Star Section, who not only persuaded the Members of his Section to undertake a considerable amount of laborious observations, but was also willing to reduce those observations and to give the Members of the Association the result of them, and to consider the physical meaning of those results. He was not content with giving them merely a light-curve, but went on to tell them what they might learn from that light-curve about the physical condition of this interesting system.

Mr. Hardcastle read a paper on "The Structure of Comets," by *Mr. Thomas Rankin.* The writer assumed: (1) the collision of meteor-streams, producing heat-centres and immense quantities of tenuous vapours formed of different substances and of varying specific gravities; (2) condensation and freezing of the tenuous vapours, and their attachment in the order of their specific gravities to the meteor stones, in the form of ice; (3) the movement of the meteor-stream towards the solar heat, the consequent vaporizing of the ice in the ice-bound section or sections of the stream, forming one or several nuclei; (4) the dispersion of the vapours in the order of their specific gravities by the radiant energy of the Sun.

The President said the subject of the paper was one that was attracting a good deal of attention just now from physicists. The hypothesis of light-pressure on very minute particles of matter seemed to explain the phenomenon of the tail. There was one point in the paper he was a little doubtful about, viz., whether ice would be deposited on these meteors in space. The gravitative power of a meteor was extremely small. However, he thought the explanation would be much the same whether the frozen matter was on the surface of the meteor or whether it was merely accompanying it in space.

Mr. Stanley E. Percival communicated a paper on "The Bright C Line in γ Cassiopeiæ," and a paper on "The Light of the Sky," by *Mr. G. J. Burns*, was also read. The writer said that on a clear moonless night, after the cessation of twilight, the sky was far from dark. The stars appeared to shine from a slightly luminous background, which gave, in fact, a much greater amount of light than the collective light of the visible stars. The light of all the stars visible at one time had been estimated as equal to that of 70 first-magnitude stars, whereas the total light of stars and sky together might be roughly taken as equal to that of 1000 first-magnitude stars. The generally received opinion was that the light of the sky was due to the light of telescopic stars not separately visible. But there appeared good reasons for believing that the luminosity of the sky by night was partly due to some other cause. If the luminosity were entirely due to the light of telescopic stars, it should diminish near the horizon just as the brightness of stars diminished, and for the same reason, viz., the greater absorption of light by the Earth's atmosphere. As a matter of fact, the exact opposite was the case. Another reason for believing that the light of the sky was partly due to some other cause than starlight was that it had been observed to vary in brightness; and if this was an actual fact it was obvious that the variable part of the luminosity must have some other origin than the light of the stars.

The President quite agreed with Mr. Burns that it was unsafe to put down the whole of the luminosity to the stars. Some might well be due, as he suggested, to tiny bodies outside our atmosphere but belonging to the solar system. But there was also the possibility that we might get some light of a phosphorescent or auroral nature produced in our atmosphere. Mr. Burns had given quite enough reason to doubt the soundness of putting all the measured luminosity of the sky down to unseen stars.

Capt. F. W. Raisin communicated a paper on "A Curious Phenomenon at Sunset."

The Meeting then adjourned at 7 o'clock.

ROYAL METEOROLOGICAL SOCIETY.

The final Meeting of the Session was held on Wednesday afternoon, June 20, at the Society's rooms, 70 Victoria Street, Westminster, *Mr. R. Bentley, F.S.A.*, President, in the Chair.

Mr. F. J. Brodie read a paper on "The Mean Prevalence of Thunderstorms in Various Parts of the British Islands during the 25 Years 1881–1905." The Author gives the mean number of days on which thunderstorms, or thunder only, occurred in each month, each season, and in each year at 53 stations situated in various parts of the United Kingdom. July is the month with the largest number of thunderstorms over Great Britain as a whole, and August at some places in the north of Scotland and north-west of England, while June is the stormiest month at nearly all the Irish stations. For the whole year, the largest number of thunderstorms is over the northern and eastern parts of England, where more than fifteen occur, while there are less than five in the west and south of Ireland and at most places in the north of Scotland. The summer distribution of thunderstorms is very similar to the annual distribution, while the winter distribution is quite different, when the largest numbers occur along the west coasts of Ireland and Scotland and extreme south-west of England.

Mr. W. H. Dines communicated a paper on a "Typical Squall at Oxshott, May 25, 1906." During the morning there was a steady wind from the south-west of over 10 miles per hour until 10 A.M., when there was some falling off for fifteen minutes, then a rise to over 20 miles per hour, accompanied by a sudden increase of barometric pressure, and a fall of a few hundredths of an inch of rain. After the squall the wind dropped suddenly, and there was almost a dead calm for about twenty minutes. The Author, who was flying a kite at the time, gave some account of the changes in the wind at a considerable altitude above the Earth. At 11.26 the squall struck the kite, which was then at a height of 8400 feet. Two minutes later, the velocity at the kite had risen to 58 miles per hour, and the wire broke under a strain of 180 lbs. Three minutes later the kite fell at a spot 2¼ miles distant from Oxshott.

The Planets and Planetary Observation.

This department of astronomy forms one of the most interesting of all, and it has always been regarded with special favour by amateurs. The study of the surface-markings of orbs similar in character to the Earth must naturally prove attractive. The determination of their exact places in the sky, their dimensions, distances, &c., involves work more suitable for professional astronomers, critically accurate measurements being required, and these are best obtainable with very large instruments provided with refined and expensive appliances.

But the observation and delineation of features visible on the primary planets do not essentially need very costly and powerful means, great experience and training on the part of the observer, or exceptionally keen vision. Certainly a really good telescope employed by a capable and well-practised eye will succeed where inferior qualifications will fail, but the chief markings on Mars, Jupiter, and Saturn are so conspicuous that they can easily be discerned by a person who looks though a moderately good instrument for the first time. To perceive extremely delicate detail requires an eye familiar with the particular object and a thoroughly effective telescope, but much depends upon the capacity of the observer. Indeed it is a well-known fact that the value of results is influenced more by the powers of the student than by the powers of the telescope he employs. A master in this field cannot be made by mere practice, be his interest in it ever so keen and his appliances ever so good. Perseverance will do a great deal, but to be eminently successful he must possess special aptitude for the work. He must also be sound in his judgment, keep his imagination entirely under control, and record such details only as are seen with absolute certainty.

In these papers * on the planets it is not intended to be hypercritical or controversial. The purpose is to follow ascertained facts as clearly as they may be discriminated and to summarize what has been accomplished as well as to suggest some points apparently needing further investigation.

Though the discs of the primary planets have been telescopically surveyed for three centuries our knowledge is still far from being complete ; in fact this field seems inexhaustible, though the objects are compartively few in number. But even one of these alone is capable of forming a lifetime study, and providing a work of very extensive kind. As an instance of what can be effected on Jupiter in a fine climate, it may be mentioned that Major Molesworth, at Ceylon, derived the rotation periods of 1000 spots and irregular markings visible during the years 1898 to 1903–4. These results involved the taking of myriads of transits ; in 1900 alone he noted no less than 6758 times of objects passing the central meridian.

The relative merits of refractors and reflectors have often been discussed, and it is not intended to reopen the question here in its bearing on planetary studies. It is quite certain that both forms of instrument are excellent and well capable of performing with thorough success in good hands. The refractor has a great advantage in retaining a consistent light-grasp, whereas the silver films of the reflector suffer deterioration and its capacity as regards faint objects varies materially according to the condition of the mirrors. But the latter form of instrument possesses several marked advantages, in the convenience with which it may be employed, in its colourless images, and in its relative cheapness. In respect to planetary definition, a speculum (figured, mounted, and adjusted

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* [This is the first of a series of papers on this subject that Mr. Denning has kindly consented to write for this Magazine.—Eds.]

thoroughly well) will bear favourable comparison with an object-glass of the very finest quality. With many amateurs price is an all-important consideration, and thus the moderate-size reflector has naturally become their working instrument, since a refractor of really adequate size—say 6 inches at least—for critical study of planetary detail would probably cost about 5 times as much as a reflector capable of equally good performance.

The effectiveness of large versus small telescopes in work of this kind has sometimes afforded the subject of argument, and the question was amply ventilated in the *Observatory* about twenty years ago. Those who use very large instruments side by side with small ones aver that the former reveal more detail and display prominent features better than the latter, and evidence gained by practical experience of this sort cannot be lightly set aside. But the difference is comparatively trivial; the possessors of small instruments have certainly no reason to consider themselves outclassed. Large telescopes are more amenable to atmospheric vagaries and disturbances, and they are less convenient to manipulate. The work accomplished by their means in delineating the configuration of the planets is really small and unimportant when compared with what has been done with telescopes from 6 to 10 inches aperture. Many thousands of drawings of Mars and Jupiter have been published at various times, and it must be confessed that the work of the 6-foot and 3-foot reflectors at Parsonstown, the 40-inch refractor at Chicago, the 36-inch at Mount Hamilton, the 26-inch at Washington, &c., &c., show nothing beyond what is readily distinguishable in an 8-inch glass. The excellent researches of Schiaparelli, which have so greatly enriched our knowledge of Mars, and the important planetary observations of Dawes were effected with small refractors. And coming to present-time experiences, it would be hard to excel the work of reflectors in the hands of Molesworth (9¼- and 12¾-inch), Phillips (9¼-inch), and Williams (6½-inch).

With regard to magnifying-power, observers usually vary this according to the atmospheric conditions, the object, the telescope, &c. It scarcely seems feasible to give a regular scale of powers most suitable for different instruments, as observers rarely agree in their inclinations and conceptions and the weather is an ever-varying feature. While some habitually employ the lowest powers consistent with a sufficiently expanded image, others use the highest magnifiers possible, preferring the larger disk even at the sacrifice of very sharp definition. For some years the writer has employed 312 on a 10-inch reflector and finds this affords the best blending of magnitude, light, and distinctness. No doubt the eye gets familiar with the frequent use of one and the same power and habit makes it really more effective than any other.

Barnard found 350, on the 36-inch Lick refractor, the most useful on Jupiter and Mars, and 520 on Saturn. Jupiter's moons, Ceres, Pallas, and Vesta would readily stand 1000. 20 or 30 diameters to the inch represented the value of the telescope on planets, and for Jupiter 10 to 15 to the inch. Hall in his observations of Saturn 1875–1889 employed the 26-inch refractor with power 383 and occasionally 606. Boeddicker in making drawings of Jupiter 1881–6 with Lord Rosse's 3-foot reflector generally used 144 and 216. Dawes in making his excellent drawings of Mars (M. N. xxv.) used an 8-inch O.G., power 258. Molesworth in observing Jupiter 1903–4 employed a 12½-inch Calver, powers 230 and 270, on Mars he applied 450. Phillips in his drawings of Mars 1903, made with 9¼-inch With, used powers 217 to 450. Williams in tracing markings on Saturn in 1903 with 6½-inch Calver found 225 sufficient. It has long been known that Saturn will bear a higher power than Jupiter; thus on occasions when 250 is found ample for the latter planet, 350 will prove more effective on the former orb*. I have usually found Mars more sharply defined than Jupiter and requiring a higher power, the disc being much smaller. But for tracing the fainter canals on Mars and the delicate "wisps" on Jupiter the powers should be moderate, or such features will be lost amid the rather diffused and feeble aspect of the image induced by very high magnifiers. In fact for the fainter planetary markings the powers should be as low as may be found consistent with the visibility of the objects. Observers will have no difficulty in ascertaining by repeated trials the power most suitable to particular objects. W. F. D.

[To be continued.]

A Danger to Greenwich Observatory.

In the House of Lords on June 21 †,

Lord Ellenborough called attention to the electrical works that the London County Council had lately built about half a mile from the Royal Observatory at Greenwich. He pointed out that the presumed position of every ship when at sea, and out of sight of land, was based upon a comparison of the results of observations taken by her officers and other observations taken years previously by astronomers at the Royal Observatory, and that the positions of all places shown on British and American maps and charts, whether European, Indian, or colonial, and also on most foreign maps and charts, were also dependent upon observations taken at Greenwich. At a conference of delegates from the principal countries in the world, 27 in number, held at Washington in October, 1884, for the purpose of fixing on a meridian proper to be employed as a common zero of longitude, 22 representatives voted in favour of adopting the meridian passing through the centre of the transit-instrument at the Observatory at Greenwich as the initial meridian for longitude, and, in consequence of this, nearly all nations of importance now use Greenwich time as a standard or universal time. If a single ship was lost in consequence of confusion being introduced into longitudes, its value might well be treble that of these new electrical works. During the last 230 years many millions of observations connected with cartography and astronomy had been made at the Observatory and other places, the value of which largely depended upon the fixity of the Royal Observatory as a reference point. The London County Council had built furnaces and tall chimneys nearly due north of the Observatory, and were using engines which caused some of the astronomical instruments to vibrate in such a manner as to destroy all possibility of accurate observation. He asked the Government whether they would apply to Parliament for power to prevent the Royal Observatory from being shaken or smoked out, either at present or at any future time. The County Council might easily have obviated the difficulty by putting down turbines or triple-expansion horizontal engines. But the geography of the whole world must not be interfered with because the County Council preferred to use vertical engines. The navigational, geographical, and business interests of the world did not permit the removal of Greenwich Observatory; and if anything had to go, it must be the County Council works. On March 13 the County Council wrote to the Astronomer Royal saying that they were "unable to acknowledge any liability in regard to the effect on the Observatory of the working of the station." It was clear that they did not realize the mischief they were doing; and it seemed sad that the education of half a million of London children should have been entrusted to a body who were evidently ignorant of the first principles of geography and astronomy. (Laughter.)

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* The earliest telescopic observers appear to have recognized that Saturn would stand magnifying better than some of the other planets, but Kitchener, who described his experiments with fifty-one telescopes in quaint and often pertinent, if not invariably accurate, terms, says: "There is a *vulgar Error* almost universally prevalent that Saturn will bear a higher magnifying power than Jupiter notwithstanding Jove's complexion is much brighter than *Saturn's*. My own experience has told me what common sense will teach any thinking mind, that *Jupiter* will bear a high power better than *Saturn* in proportion as he shines with more vivid light." There are other points, however, besides mere relative brilliancy which materially affect the definition of different objects.

† [This debate arose from the fact that the London County Council have built a station for the generation of electricity on the south bank of the Thames, the centre of which is not many feet east of the meridian of the Greenwich transit-circle, two very tall chimneys being a prominent feature. Within the last few months the station has been enlarged beyond previous expectation, and the erection of two other chimneys has been begun, one of which is in the meridian of the altazimuth, and whose top, if the height is to be the same as those already existing, will be above the horizon. Experiments have been made

Lord Tweedmouth said that since the subject had been raised it had been closely considered at the Admiralty. He had been in conference with the authorities at Greenwich and with the representatives of the County Council. As to the origin of this generating station, in 1901 the London County Council resolved on it, and in 1902 a Bill was passed through Parliament. In this Bill was inserted a clause known as the Observatory clause, which gave to the Board of Trade the power, if any use of electrical power was likely to affect injuriously the instruments used in the Observatory, to require reasonable and proper precautions to be taken. This proposal was made public and approved by Parliament. It was a pity that the County Council had not more closely apprehended the possibility of danger in choosing this particular site, but he thought that there was some responsibility also to be attached to the various departments and also to Parliament. He thought that the noble lord had given an exaggerated account of what had taken place. That afternoon he had a

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with the transit circle and with portable transits by observing the image of a spider wire after reflection at the surface of mercury, and in the transit-pavilion, where the portable transit is mounted, the reflected image is clearly seen to be in continuous vibration when the engines at the generating station are running. Popular opinion asks why complaint was not made before the station was erected. The answer to this is that when the original scheme was submitted to the Astronomer Royal it was not anticipated that the engines would be of such a size and type as to cause the effect now seen.—Eds.]

conversation with the Astronomer Royal, and he thought that at present at any rate no absolute damage had been done. He admitted, however, that there was an apprehension of it when the station was developed to its fullest power. It would be almost the biggest generating station in the world when completed. Eight engines would work up to 52,000 horse-power, and the electricity generated would be sufficient to work the whole of the London tramway system. But at the present time the Astronomer Royal said that no serious effect had as yet arisen in the working of the principal meridian instrument. This was a stationary instrument not easily subject to vibration. The Astronomer Royal, however, said that the instrument which had been affected was the portable transit instrument used for determining longitude. There had thus been evidence of some disturbance in this instrument, but the Astronomer Royal said that the appearances were variable and intermittent, and sometimes stopped altogether. The Astronomer Royal added that from the large generating station at Deptford no damage had resulted, and there was no indication of any disturbance. What the authorities had to do was to take very careful observation as to what was exactly going on at Greenwich. At present he believed that the station was never worked up to more than 3000 horse-power. A trial had been made of two engines, but the experiments were neither complete nor satisfactory. What he proposed to do was to ask Professor Ewing, who lived at Greenwich, a distinguished engineer intimately acquainted with scientific instruments, to represent the Admiralty in the observations to be taken, and which must extend over a considerable time. He was told that the disturbances varied very much, and that there was a great deal to be said as to the possibility of meeting the difficulties by reducing the high chimneys, though the Astronomer Royal did not think that the vapour of the chimneys seriously interfered with the observations. He proposed also to ask the County Council to appoint a representative of their own for observation in order to have an independent report as to the exact amount of disturbance that might arise. He had had a conference with the members of the County Council, and that body had expressed itself very desirous to meet the wishes of the Admiralty and to do anything it could to make such alterations in the plant as would prevent damage to the Observatory.

Viscount Goschen thought that the statement of the noble lord must fill any one with alarm. One of the questions which suggested itself, however, was whether the County Council in the meanwhile were going to continue this gigantic work? Were there no means of coming to an arrangement with the County Council that it should not continue to spend large sums of money upon these works? Evidently a gigantic mistake had been made—a mistake by the Admiralty, by the Astronomer Royal, by the County Council, and by Parliament, and he did not gather from the tone of the noble lord the slightest note of confidence that it would be possible to remove all the objections that had been raised. The matter affected not only the Greenwich Observatory, but the whole scientific world. In these circumstances he scarcely thought that the device of continuing the experiments and watching the observations was quite adequate. The greatest scientists in the kingdom should be associated in some form or another with this work, so that the country and all those interested should be assured that every scrap of scientific knowledge available in this country of whatever class was being brought to bear on the subject. The idea that Greenwich Observatory, which had rendered so many services to the world, should be imperilled even in the slightest degree, even by a varying interference, was really a prospect which few persons could contemplate with equanimity.

The Earl of Crawford dealt with two aspects of the question—first, the chimney, and second, the effect of the machinery when set in motion in the generating station. He examined the effect of the chimney in its relation to the refraction of light, and illustrated his meaning by describing what took place when the orb of the Sun as seen from the deck of a steamer touched the water-line on the far horizon. It had been suggested that there might be a possibility of lowering the chimneys, but he did not think that would have any very great effect. Reference had been made to the mistakes made by many and various public departments, but there was one mistake which ought to be noticed in this connexion, and that was that the architect who placed this building for the London County Council in its present position was an architect who had been employed at the Royal Observatory. The chimney was absolutely on the meridian of one instrument, although it might easily have been placed in a very different position. With reference to the question of tremor, he said that the shaking was so small that the ordinary person might treat it with contempt; yet the movement was very large when taken in connexion with the delicate observations which had to be carried out at Greenwich. The essentials for observation of an astronomical kind were stability and quietude. Nothing was so dangerous in astronomical observation as the unknown errors which had to be guarded against at the present time. If an error was known no great harm was done. In connexion with the meridian careful observations of the Moon required to be made. For many many years the Moon had been given over by the scientific world to Greenwich Observatory. The Royal Observatory had specialized on the Moon mainly owing to the grand labours of Sir George Airy, the late Astronomer Royal, so that the position of the Moon at a given time hence might be fairly accurately predicted. The observations at Greenwich and the manner in which they had been carried out by the late Astronomer Royal had led the whole scientific world to say, Gentlemen, you know your Moon so well, pray continue to be responsible for her. In the same way the observation of the minor planets rested with Berlin, and other matters were looked after by American observers. If now Greenwich were reduced to the position of saying that its lunar observations had not the weight and value which so far had attached to them, it would be a terrible blow to the reputation of the Royal Observatory and also to our existence as a scientific country. Another difficulty was that disputes as to boundaries between countries were mainly settled by astronomical observation as to the position of the Moon; and as the Moon was being constantly watched at Greenwich Observatory, applications were frequently received from foreign countries as to the error of the Moon at such an hour on such a day. That also showed how extremely important it was that the observations at Greenwich should be reliable. Such was the accuracy demanded of the instruments at Greenwich Observatory that measures of extension were fractions of thickness of a spider's web and hundredths of a second of time. Therefore, though the disturbance caused to the instruments at the Observatory by the electric works might be infinitely small, that infinitely small disturbance might lead to incalculable damage. (Cheers.)

LORD KELVIN said they were all deeply indebted to the noble earl who had introduced this subject and to Earl Crawford for his very interesting speech. They must all be deeply impressed with the gravity of this question. The difficulties of the situation were absolutely endless. But they were comforted by the assurance of the First Lord of the Admiralty that whatever institution was removed from Greenwich it should not be the Royal Observatory. It was most undesirable that the site of the Royal Observatory should be changed. On the other hand, there was a serious responsibility on account of the amount of ratepayers' money which had been spent by the London County Council. If there was any solution by which the electric works at Greenwich could go on without seriously threatening or unduly interfering with the work of the Observatory the whole public would be glad. (Hear, hear.) Even at present, he thought, they might look forward to possible changes in the arrangement of the works by which the electricity might not seriously disturb or practically cripple the astronomical observations at Greenwich. He did not know how far the County Council works had progressed.

LORD TWEEDMOUTH said that about half the works had been erected.

LORD KELVIN said he was glad to learn that there was still time to avert half the possible damage. He suggested that the disturbance caused at the Observatory by the vibration from the electric works might be to some extent avoided by the substitution of steam turbines for reciprocating engines and the use of different electric dynamos. It was no exaggeration to say that the whole world outside, as well as the British Empire, would deplore anything that would injure the great and good work that was done in the Royal Observatory at Greenwich, and both Houses of Parliament should unite in preserving it.

Earl Cawdor thought the statement of the First Lord of the Admiralty was not quite satisfactory. What they wanted was to guard most carefully against the slightest increase in the evils which were admitted to exist. He hoped the noble lord would be able in a short time to assure them that some further steps were being taken. (Hear, hear.) The first step to be taken, if it was possible to do so, was to stop any further expenditure by the London County Council that might be found to be useless, or worse than useless. It appeared certain that any further development of the powerful engines that were being used must increase the evils. He did not not know whether there was any legal process by which the County Council could be estopped from carrying out works that did mischief to the Observatory, but, if not, he thought powers should be obtained. He hoped the question of cost would not be allowed to stand in the way. The half-million of money expended by the County Council was comparatively a small item when it was remembered what was at stake.

Lord Tweedmouth said the Law Officers had had the whole question of the powers of the Admiralty and the Board of Trade before them, but this subject had really only arisen in the last few days. He had also consulted with Lord Rayleigh. The leading astronomers of the country would also, of course, give their assistance. The London County Council had told him that they would not go on with the two chimneys, which were now only partly erected, and he imagined that there would not be much more done with regard to the installation that existed. He held that it was desirable to discover what the effect of the existing installation really was. It was, he thought, necessary that that installation should be worked at its full power in order that they might discover what was the effect on the instruments at Greenwich. He thought the House would agree that before doing anything they were bound to discover whether by any rearrangement of the machinery the threatened damage could be averted —for he could not admit that up to the present moment it was more than a threat of damage. He undertook that every effort should be made to make the inquiry a thorough one, and one which should command every one's respect.

In reply to the **Bishop of Southwark**,

Lord Tweedmouth was understood to say that the existing installation had 3200 horse-power, and that the complete installation contemplated would have 52,000 horse-power.

The **Bishop of Southwark** said that was an enormous development.

The Times, 1906 June 22.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Eclipse of B.C. 1063.

Gentlemen,—

Objection has been raised to Mr. Cowell's identification of the earliest of the eclipses on his list with that of B.C. 1063 on the ground that "the 26th day of the month Sivan" could not fall so late as July 31. It is asserted, and generally assumed, that Sivan, the third month of the year, must have coincided with May or June, or with parts of both of these two months, and could never have fallen so late as to extend to the beginning of August.

There are certain points which we learn from the tablet in question that are quite irrespective of the year when the eclipse took place. First, we learn that the beginning of the month Sivan had not been fixed by actual observation of the New Moon, otherwise the eclipse would have been recorded as taking place on the 28th or 29th day of the month. Similarly, we infer that the beginning of the previous month, Iyar, the second month of the year, had not been fixed by actual observation either; and it is possible, indeed probable, that the same was the case with Nisan, the first month of the year. The last month of which the beginning had been fixed by actual observation was, in all probability, the concluding month of the preceding year—Adar or Ve-adar, according as that year was one of twelve or of thirteen months.

Next, we learn that when the Babylonians had to go by "dead reckoning," as a sailor would say, or "to count by hand," as they themselves would express it, they reckoned thirty days to each month, and did not alternately allow twenty-nine days and thirty days, as Solon arranged for the Athenians. The Babylonians had therefore either not yet realized that the mean month was half a day short of thirty days, or they did not think the error in their assumed value worth notice. In either case their astronomy must have been of quite a rudimentary character.

The above considerations are independent of the date of the tablet. A third and more important point is intimately connected with its date.

We know of two methods by which the month Nisan, the first month of the year, was recognized. The first method was that of the seleniacal setting of Capella; if this occurred on one of the first three evenings of the month, that month was Nisan*.

This first method had a great antiquity. We do not know when it was superseded by the second method, but the second method was in use in the seventh century B.C. The second method consisted in the determination of the equinox by the use of some mechanical method of measuring time which enabled the length of the day to be recognized as equal to the length of the night †.

If the second method were in operation at the time of the tablet, Sivan 26 could not possibly have fallen so late as July 31. For the equinox fell on March 31 in the year 1063 B.C., and the apparent equinox two days earlier, on March 29. The conjunction of the Sun and Moon took place on April 5, and the earliest observable crescent Moon on April 6. It is not possible to suppose that the Babylonians, if they used this method, can have imagined that the equinox was yet to come when it had been already past by eight days.

But we have no evidence that this method dates back earlier than the seventh century B.C.; and as the tablet itself indicates quite a rudimentary state of astronomy, it is highly probable that the earlier and simpler method was still in use.

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* "Preliminary Paper on the Babylonian Astronomy." By R. H. M. Bosanquet, Esq., and Prof. A. H. Sayce. *Monthly Notices R. A. S.* vol. xxxix. p. 455.

† "Note on the Date of the Passage of the Vernal Equinox from Taurus into Aries." By E. Walter Maunder and A. S. D. Maunder. *Ibid.* vol. lxiv. p. 501.

As the observed New Moon would fall on April 6, and the seleniacal setting of Capella on April 9, *i. e.* on the fourth evening of the month, this method would mark the month in question as the last month of the old year, not as the first of the new.

The next occasion when the New Moon had drawn far enough away from conjunction to be observed would be May 5, and the seleniacal setting of Capella would take place the same evening, marking the month then beginning as Nisan. But it is clear that the New Moon of May 5 was not actually observed, for then the eclipse of July 31 would have been recorded as falling on Sivan 27. The crescent of April 6 must therefore have been the last actually observed previous to the eclipse; in which case, the months being "counted by hand," thirty days to the month—the cuneiform expression for "month" signifies "thirty"—Sivan 1 began on the evening of July 5, and Sivan 26 began on the evening of July 30 and lasted till the evening of July 31.

The objection to Mr. Cowell's identification rests, therefore, solely on the assumption that the method of determining Nisan 1 by reference to the equinox was in operation some four hundred years earlier than we have any evidence to show that it was. The method which we know to have been in operation earlier, the seleniacal setting of Capella, exactly confirms his identification, and corresponds in its simplicity to the rudimentary state of astronomy indicated by the tablet.

Faithfully yours,
E. WALTER MAUNDER.

86 Tyrwhitt Road,
St. John's, London, S.E.

Count Cassini.

GENTLEMEN,—

The long line of this astronomical family terminated with Jacques Dominique, Comte de Thury, for though his son, Alexandre Henri Gabriel, obtained celebrity in science, it was of a different department, that of botany, and he died of cholera in 1832, thirteen years before his father's death.

The first Cassini, Giovanni Domenico, was born at Perinaldo, near San Remo, in 1625; he was Professor of Astronomy at Bologna until summoned by Colbert in 1669 to be Director of the Observatory which he had founded at Paris and was not completed until 1672. The Count, his great grandson, published his autobiography in 1810, from which we learn several interesting particulars respecting his early career. Noteworthy is his rejection of judicial astrology, after reading the work of Mirandola on the subject, subsequently to which (apparently at Cassini's instance) Father Noceto preached against it in the Church of St. Ambrose, Genoa, and obtained a great triumph when, Orderigo having predicted in an almanac the occurrence of a storm which did actually take place, he proceeded to foretell another and so frightened the people that numbers rushed away from their homes on the approach of the day indicated, the weather on which turned out to be a dead calm. The first J. D. Cassini did not accept the Newtonian philosophy, and his erroneous ideas on the figure of the Earth were called attention to in an interesting address given to the May meeting of the British Astronomical Association. It is well known that Delambre considered that Lalande's eulogium on the value of his work to the progress of astronomy was exaggerated, and that a different line of action would have contributed much more to the progress of astronomy.

His son, Jacques, the second Director of the Paris Observatory, is probably best known by his 'Elémens d'Astronomie,' which appeared in 1740; and his son, César François Cassini de Thury, the third Director, for commencing the great map of France, which was completed by his son, the fourth, who afterwards accepted the title of Count from Napoleon, the place being a seat near Clermont, in the Department of Oise, which his father had acquired.

The last of the Cassinis was born at Paris in 1748, and had exceeded ninety-seven years of age when he died on the 18th of October, 1845. Troublous times, indeed, came upon the Paris Observatory before he left it. His 'Mémoires pour servir à l'histoire des sciences et à celle de l'Observatoire royal de Paris' (to which, amongst other things, is appended the autobiography of his great ancestor to which I have already alluded) appeared in 1810. In this he describes the energetic efforts made by him to restore the buildings of the Observatory, which had fallen into sad neglect and decay during the latter part of the reign of Louis XV., in which he was supported by Count d'Angivillers, Director of Public Buildings in the early part of the reign of Louis XVI. It was his great desire also to supply it with suitable instruments of modern type, which the French artists of that period were not able to produce. Having occasion to go to England, with Méchain and Legendre, on the commission for connecting the triangulation of France with that of England, he made the acquaintance of the famous Ramsden, of whom he speaks very highly, and ordered instruments from him, the principal one of which, however, did not arrive until some years after Cassini had left Paris; but this was due to the revolutionary troubles. How sorely these distressed him and hindered the latter part of his operations he tells us in his book. During his visit to England he did not fail to pay a visit to Sir W. Herschel at Slough; indeed, as he says, to have done otherwise would have been like going to Egypt without seeing the Pyramids. He describes how energetically that great astronomer worked, with the constant assistance of his sister, particularly at the observations of nebulæ; the principal instrument was the 20-inch reflector; the 40-inch was schemed, but not then finished.

Cassini's Directorship of the Observatory terminated in 1793. On the 30th of August in that year the National Convention decreed that it should not have one head, but four of equal authority. One of these would be Cassini, the other three pupils of his; well aware of the intrigues which led to this arrangement, he says:—"Je crus prudent et indispensable de céder la place et d'envoyer ma démission." On this another was appointed as fourth, "qui," says Cassini, "sans doute s'est montré depuis digne d'occuper un des premiers observatoires de l'Europe, mais dont le choix, à cette époque, fut, j'ose le dire, d'autant plus extraordinaire, qu'il y avait à peine six mois qu'il s'occupait d'astronomie." The good choice, as it turned out, thus made was of Alexis Bouvard, afterwards Director. The other three to whom the divided directorship was confided were Citizens Nouet, Perny, and Ruelle, and this arrangement subsisted until the 17th of May, 1795, when the Committee of Public Instruction appointed Lalande sole Director, a post which he held till his death in 1807.

During a large part of 1794 (for 7½ months), Cassini was detained in custody as a royalist; his papers were examined and dispersed, and after his release he returned to his seat at Thury, and, though he subsequently became a member of the Institute, he took no further part in astronomical observation. He was sixty-two years of age when he published the 'Mémoires' from which I have quoted. Besides the autobiography of Cassini I. and a list of his works and papers, they contain *éloges* of Maraldi, Le Gentil, and Saron.

Yours faithfully,

Blackheath, 1906, June 1. W. T. LYNN.

The Definition of a Drought.

GENTLEMEN,—

In the interest of uniformity in terminology, I trust that you will not encourage the introduction of a new meaning for the term "absolute drought," now well established as referring to a period of more than 14 consecutive days on no one of which so much as ·01 inch of rain has been recorded. I freely admit that the expression *absolute* is inappropriate, but it has been applied and it has become usual. The phrase "absolute drought" may, I think, be taken, as a whole, as meaning a period of more than a fortnight without one rainy day. And as the ordinary rain-gauge in the hands of the ordinary observer cannot be read more closely than to hundredths of an inch, it seems better to continue to use the established form of words in the customary sense than to introduce a new "Greenwich limit" of ·001 inch (p. 224 of your May number).

Although I, in common with most scientific observers, read rainfall to the third decimal of an inch, it seems to me advisable to publish it only to the second. Whether a fall of ·001 or ·002 inch will be recorded by a rain-gauge or not is very uncertain. If the surface of the funnel is warm and the air dry, so small a quantity would certainly be evaporated without a drop entering the receiver, while if the funnel is cold and the air nearly saturated a small fall of this kind would be perhaps doubled by moisture condensed directly upon the metal. Rain also descends patchily, and not in a homogeneous sheet, so that in a group of rain-gauges standing within a few feet of one another it is rare to find the readings, even in steady rain, accord to ·001 inch or anything like it.

In India the limit which must be reached for a day to be accounted as one with rain is ·10 inch; in this country the limit of ·01 inch has been universally adopted by meteorologists on the understanding that anything more than ·005 is recorded as ·01, and anything less than ·005 is ignored. I do not think that, taking into account the physical peculiarities of atmospheric precipitation and the imperfections of even the best rain-gauges, we ought to give any weight to differences of rainfall less than one-hundredth of an inch, though, of course, it is easy enough to read to thousandths, and I have one correspondent who regularly sends in his return worked out to ten-thousandths of an inch.

62 Camden Sq., London, N.W.,
1906, May 19.

Yours sincerely,
HUGH ROBERT MILL.

OBSERVATORIES.

ROYAL OBSERVATORY, GREENWICH.—For the first time within memory Visitation Day at Greenwich did not happen on a Saturday. It has lately been arranged that on occasions when the first Saturday in June happens to be the day before Whit Sunday, the President of the Royal Society, may, if he wish, transfer the annual ceremony to some other day, because of the modern custom of leaving town at Whitsuntide. This year, therefore, the Board of Visitors came to Greenwich on Wednesday, May 30, together with a general company of about one hundred and fifty. The following details are extracted from the Astronomer Royal's Report.

Transit-Circle.—The number of transits observed during the year was 6335, of Circle observations 5531. These figures are notably smaller than the similar ones in previous reports, the diminution being due to the fact that the observations for the Nine-year Catalogue were nearly complete at the beginning of the period to which the report refers, and that the object-glass of the instrument was dismounted for repolishing for two months.

The apparent correction to zenith-point for discordance between observations of the nadir and stars by reflexion was found to be −0″·22. The values found since 1892 range from −0″·20 to −0″·41. The correction for the R−D discordance was found to be +0″·055 + 0″·310 sin z.d., agreeing well with the results of previous years, and a diurnal period of change of level and nadir-point of the instrument still continues to be shown by the observations.

The correction to the tabular obliquity of the ecliptic as found from observations of the Sun in 1905 was $+0''\cdot21$. The apparent correction to the Right Ascensions of clock-stars found from the same observations was $+0^{s}\cdot038$. In determining these quantities the correction for latitude variation has been applied, as it also is in completing all reductions of stars and planets. The mean error of the Moon's tabular place computed from Hansen's lunar tables with Newcomb's corrections is $-0^{s}\cdot337$ in R.A. and $-0''\cdot45$ in N.P.D. It may be noted that the error of R.A., which has been growing in recent years, shows a still further increase.

The Altazimuth.—This instrument continues to be used for observations of positions of Sun, Moon, Planets, and fundamental stars, sometimes in the meridian as a transit-circle, and on other occasions with the telescope in some other vertical plane. During the year ending 1906, May 10, which is the period covered by this report, 1294 observations of R.A. and 1156 of N.P.D. of the Sun, planets, and stars have been made, with 78 and 52 observations in R.A. and N.P.D. respectively of the limbs of the Moon, the telescope being in the meridian, the number of observations of our satellite off the meridian being 33. When the instrument is used as a transit-circle on the meridian, besides the limbs of the Moon, the crater Mösting A is also observed, and this has been accomplished 50 times. These observations of the Moon in all cases appear to be very satisfactory, judging from the accordance with the results found from observations with the transit-circle.

Taking both instruments, observations of the Moon have been obtained on 136 days during the year.

The Reflex Zenith Tube.—During the year 823 double and 24 single observations were made with this instrument, the number of stars being 62.

The Equatorials have been used for the same purposes as in former years. Twenty-six disappearances and six reappearances of stars occulted by the Moon were observed by one or more observer. With the 28-inch refractor, the number of measures of double stars was 606; of the stars observed 158 had their components separated by less than $1''$ and 70 by less than $0''\cdot5$. The measures of δ Equulei leave little reason for doubting that its period is only 5·7 years. The 26-inch refractor and the 30-inch reflector of the Thompson equatorial have been used for photographing Neptune and its satellite, Jupiter and its Satellites VI. and VII., comets, minor planets, and nebulæ.

The printing of the measures of the Astrographic Catalogue is almost completed, and since the Greenwich Nine-year Catalogue from observations with the transit-circle is nearly finished, the places of some of the reference stars are now available and the work of determining plate-constants has been begun.

The solar activity, as shown in the numbers and areas of sun-spots, was very pronounced throughout the whole of 1905, the record for the year being about double that of 1904; in particular a great number of large groups, visible to the naked eye, were observed. An arrangement has been made to receive solar photographs from the Kodaikanal Observatory for days on which it has not been possible to photograph the Sun either at Greenwich or Dehra Dun. In the rare cases when the record fails at the three observatories, the series will be completed by the Mauritius Observatory.

The mean temperature for the year 1905 was 49°·7, or 0°·2 above the average for the 50 years 1841–90. The highest temperature in the shade recorded (on the open stand in the magnetic pavilion enclosure) was 87°·2 on July 26. The lowest temperature of the air recorded was 23°·1 on November 22. There were 59 days on which the temperature fell below 32°·0, which is three more than the average number.

The Astronomer Royal closed his Report with some remarks on a subject connected with the welfare of the Observatory, which is referred to in another part of this number.

PUBLICATIONS.

THE RADCLIFFE CATALOGUE.—To those who work in observatories at what may be called, for want of a better name, fundamental astronomy, the title of this note has been a household word for many years past. There is a First Radcliffe Catalogue for epoch 1845, a Second Radcliffe for epoch 1860, both edited by the Rev. R. Main, and then, *longo intervallo*, another compiled by Mr. Stone, for epoch 1890, which generally goes by the name of the New Radcliffe, or, possibly, Stone's Radcliffe. And now there has come another, which will probably be familiarly called Rambaut's Radcliffe, but whose full title is "A Catalogue of 1772 Stars chiefly comprised within the Zone 85°–90° N.P.D. for the Epoch 1900, deduced from Observations made at the Radcliffe Observatory, Oxford, during the Years 1894–1900, under the Direction of Arthur A. Rambaut, M.A., D.Sc., F.R.S., Radcliffe Observer" *.

The instrument with which the observations were made was the Transit-circle made by Messrs. Troughton & Simms for Mr. R. C. Carrington, and is the one with which his observations contained in the Redhill Catalogues were made. It has an object-glass of 5 inches aperture and 66 inches focal length, and the divided circle on which the divisions are traced is 39 inches in diameter. Shortly after Dr. Rambaut went to Oxford, in 1897, he began to investigate the stability of the instrument, to make improvements in this respect and others, and to make additions. The improvements consisted largely in disconnecting the piers which supported the instrument and the collimators from the floor; the additions were a new and improved breech-piece carrying a drum-micrometer for recording declinations, made by Messrs. Troughton & Simms, and a chronograph of Sir Howard Grubb's latest pattern, which things having been added to an instrument already of some reputation, and this being placed in the hands of such capable observers as Messrs. Wickham, Robinson, and McClellan, it was to be expected that the result would be a work of some excellence.

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* Oxford University Press, 1906. Price 15s. net.

The review of a work of this kind proceeds almost by formula—one looks for certain points in which one catalogue differs from another. The Right Ascensions were in the first instance based on the Greenwich Clock Star Lists, which, it may be known, were formerly deduced from Greenwich Observations, but which since 1903 have been similarly deduced, but have been corrected by the addition of $0^{s}{\cdot}054$ to refer them to an equinox adopted by Prof. Newcomb. The Sun was observed on the meridian at the Radcliffe Observatory in the years 1896–1899—it is not stated why only in these years—and from these observations Dr. Rambaut has derived positions of the equinox which are not very accordant in the several years, but which give an almost insensible correction in the mean, so that no correction has been applied on this account, and as the correction to Newcomb's equinox has not been adopted, the equinox of the catalogue is that of the Twelve Hour Groups in the Greenwich Second Ten Year Catalogue for 1890.

The observations for North Polar distance consist, in the first instance, of measurement of distance from a zenith 180° from a nadir point determined by means of a Bohnenberger eyepiece. The zenith-distances of some stars are observed by reflection, and comparison of these observations with those of the same stars made "direct" show a difference which can be represented by the formula

$$R - D = +0''{\cdot}295 - 0''{\cdot}184 \sin \text{z.d.};$$

but this has been considered due wholly to the reflection observations, and, the second term being neglected, as it rests on somewhat slender evidence, the constant has been applied to correct all reflection observations to render them homogeneous with the direct. The colatitude to which these zenith-distances have been added to form N.P.D. has been deduced from 246 close circumpolar stars observed above and below pole, to which observations correction for variation of latitude was not applied, as such correction appeared to have an effect of only $-0''{\cdot}01$ on the result; nor was correction for variation of latitude applied to individual observations.

Remembering the ingenious device arranged by Dr. Rambaut for determining the form of pivots, it is not surprising to read that the observations of R.A. have been corrected for irregularities thus found. Level error was obtained by means of a mercury trough and reflecting eyepiece. Collimation by north and south collimators. In the formation of the catalogue the proper motions adopted are in most cases those found by Dr. Auwers from reduction of Bradley's observations.

It seems unnecessary to describe the separate columns of the catalogue in detail, for they follow the plan usual in such works, but there are some additions. The magnitudes of the majority of the stars have been taken from the Harvard or other well-known publications, but the magnitudes of some stars depend on observations made at Radcliffe. In such cases the number of estimations is given. There are seven columns giving references to other catalogues. The accuracy of the results can be gauged from the fact that the Probable Error of a Catalogue R.A. depending on three observations, which is the case for most of the stars, is

$$\pm 0^{\mathrm{s}}{\cdot}016 \times \operatorname{cosec} \text{N.P.D.},$$

and of a Catalogue N.P.D. of a star similarly observed $\pm 0''{\cdot}18$.

But, perhaps, a better criterion of the methods of reduction is supplied by the comparisons with other catalogues. First, the comparison with Newcomb's Fundamental Catalogue shows that the R.A. of the Radcliffe is small by $0^{\mathrm{s}}{\cdot}052$, which is not surprising when it is remembered that the R.A. of Newcomb's equinox is, as above stated, larger than that of the Greenwich Catalogues by about that amount. The mean N.P.D. of the Radcliffe Catalogue is smaller than that of Newcomb by $0''{\cdot}30$, which at first sight seems as though it might have some connection with the adopted colatitude of the Radcliffe Observatory. The colatitude used in this catalogue is $38^\circ\ 14'\ 24''{\cdot}41$, which is exactly $0''{\cdot}20$ smaller than that used by Stone in the 1890 Catalogue; and as the number of observations on which the new value depends is not excessive, it might be thought that its adoption would lead to discordances with other catalogues, but then, on the other hand, comparisons between the North Polar Distances of the Radcliffe Catalogue 1900 with that of Radcliffe 1890 shows—for stars south of the zenith at least—that those of the former catalogue are the larger, which is not consistent with the above suggestion. Comparison with the Greenwich Catalogue 1890 produces little difference in Right Ascension, which was to be expected, since they depend on the same clock star system; but the North Polar Distances of the Greenwich Catalogue are, in the mean, the greater by $0''{\cdot}60$. Dr. Rambaut says this is due to the fact that a correction which goes by the name of R−D, and whose mean value is about $+0''{\cdot}60 \sin$ z.d., has been applied to the Greenwich Zenith Distances. We are not called upon here, in this review, to justify the Greenwich practice, but quote Dr. Rambaut's figures to show the size of possible systematic errors of these catalogues. Sufficient has no doubt been written to show with what care all points of procedure have been considered, and to make it clear that this catalogue is no mean contribution to fundamental astronomy.

NOTES.

Comet Notes.—M. Schulhof publishes in *Ast. Nach.* 4100 a search ephemeris of Finlay's Comet for this year:—

	R.A.	S. Dec.		R.A.	S. Dec.
	h m s	° ′		h m s	° ′
July 1 ..	22 28 10	17 45	July 21 ..	0 17 48	10 32
5 ..	22 45 14	16 51	25 ..	0 48 8	7 59
9 ..	23 4 25	15 45	29 ..	1 21 4	5 0
13 ..	23 26 3	14 22	Aug. 2 ..	1 55 56	1 42
17 ..	23 50 27	12 39			

Perihelion passage Sept. 8·0.

The comet on June 18 was twice as bright as when discovered in 1886, and it is steadily brightening till towards the end of August. There is therefore a considerable prospect of its being observed.

A. C. D. C.

Minor Planet Notes.—The following planets have been named:—

538 Friederike.
545 Messalina.
547 Praxedis.
548 Kressida.
556 Phyllis.
563 Suleika.
564 Dudu.
567 Eleutheria.

The long missing planet 294 Felicia, last observed in 1891, was recovered by Prof. Wolf on May 23. He discovered a new planet UL (mag. 12·6) on May 29.

Planet 526 is identical with HB, discovered in 1901 (the previous conjecture of identity with HA was wrong).

The *Astrophysical Journal* for May contains an article by Mr. J. H. Metcalf on his method of discovering asteroids. His object is to get a nearly circular image of the asteroid and trails for the stars, so he moves his plate parallel to the ecliptic at a rate computed for an average asteroid. The movement is given by hand every minute, but it might be made automatically. Two images on the plate (with a small shift in declination) enable planets to be distinguished from photographic defects. The method has been very successful, and several new planets have already been discovered. The plates lend themselves to accurate measurement, the asteroid images being nearly circular, while stars with well-defined trails can be selected as comparison stars. Some interesting reproductions of photographs accompany the article.

A. C. D. C.

The Cause of Earthquakes.—It is rather late to refer to the San Francisco earthquake, but the occurrence of a rather severe shock in Wales on June 27 may be considered a reason for describing an hypothesis which formed the subject of an article by Prof. Turner in *The Times* soon after the Californian disaster.

It has been shown by M. Poincare, Sir George Darwin, and others, that under certain conditions of elasticity, density, and speed of rotation, the form of a rotating body would not be that of a spheroid. In his British Association address last autumn, Sir George Darwin described the different stages in the shape of a rotating body: that being an ellipsoid it might become elongated with bulbous ends, like a dumbbell; that one end might break off and form a satellite, leaving the parent satellite as a body more or less pear-shaped. Now Mr. Jeans, of Cambridge, in a remarkable paper in the Phil. Trans. of 1903, showed that for the Earth the spheroidal form was one of stability, but suggested that our planet retained some traces of a pear-shaped form, and was attaining a spheroidal form by occasional ruptures, which appear as earthquakes, and take place in places of greatest weakness, which may be reasonably supposed to be located at the equator of, and not at the blunt and pointed ends of the pear. Prof. Sollas, of Oxford, following up this idea, takes the land hemisphere, of which the middle of Africa is the centre, to be the blunt end of the pear and the stalked end to be represented by a tract of land formerly occupying part of the Pacific Ocean for which some evidence exists. These two points happen to be the centres of two rings, the radii of which are respectively 65° and 50°, on the circumference of which are found the regions where earthquakes are prevalent.

A Trans-Pacific Longitude.—Just a year ago an article appeared in this publication giving the result of a determination of longitude by officers of the U.S. Coast Survey, through the cable of the Commercial Pacific Cable Company, from San Francisco to Manila. It was there mentioned that officers of the Canadian Survey also had measured a longitude arc through the State-owned Pacific Cable from Vancouver to Australia and New Zealand. The result of this work without details has just been published in a descriptive article by Prof. O. Klotz, who directed the undertaking and took part in the observations, in the 'Transactions of the Royal Astronomical Society of Canada' for 1905.

It will be seen from the foregoing paragraph that the terminal stations of the two Pacific longitude arcs are different at both ends, and therefore to make comparison of, or rather to combine, the results, it is necessary to form a circuit by joining the ends. Or an interesting and more convenient comparison can be made by forming the longitude from Greenwich of some place—say Madras—by different routes, of which the two arcs in question form part. Thus, measuring westward, differences of longitude are :—

	h	m	s
(1) Greenwich to Vancouver	8	12	28·368
(2) Vancouver to Sydney	5	42	42·345
(3) Sydney to Madras	4	43	50·120

Therefore

Greenwich to Madras .... 18 39 0·833 Westward.

The arc (1) is the result of a trans-Atlantic determination made in 1892 by Greenwich observers in co-operation with Canadian, added to a network of the Canadian Survey across Canada, (2) is the Pacific longitude now being written about, and (3) is an arc *viâ* Port Darwin and Singapore given by the Directors of the Australian observatories as the result of a longitude campaign in 1883 and 1884. By reference to the number of this Magazine for last July, it will be found that by the other cable across the Pacific the longitude of Madras is 18^{h} 39^{m} 0^{s}·574 west of Greenwich, a sufficiently close agreement, remembering how many separate arcs go to make the whole in each case. Subtracting these from 24 hours to find longitude east of Greenwich and supplying direct eastward determinations for comparison, we have the following values for the longitude of Madras:—

	h	m	s
By the All-British Cable	5	20	59·167
„ Comm. Cable Company's Cable	5	20	59·426
Viâ Vladivostock and Singapore *	5	20	59·452
„ Berlin, Malta, Aden, and Bombay †	5	20	59·233
„ Mokattam, Aden, Bombay	5	20	59·421
„ Potsdam, Teheran, Karachi	5	20	59·113
„ „ „ „	5	20	59·246

* Prof. Oudemans's result. † Prof. Auwer's result.

These seven routes are not entirely independent. The arc Singapore to Madras, for instance, enters into the first, second, and third, and the determination Aden to Madras forms part of both the fourth and fifth chains. The sixth result was found by officers of the Indian Survey, and the last is the same result corrected for the latest adopted value of the arc Greenwich to Potsdam. The astronomical value of the longitude of Madras now adopted by the Indian Survey is 5^{h} 20^{m} 59^{s}·137, the geodetic value being 5^{h} 20^{m} 59^{s}·60.

THE CANADIAN R.A.S.—The annual volume of publications of this Society contains, besides the longitude result referred to in a previous note, other interesting papers on various subjects. One describes a research leading to the conclusion that there is a Lunar Tide on Lake Huron. Another collates some information under the heading "Stellar Classification." Another attempts an explanation of the hypothesis put forward by Dr. C. L. Poor, that the Sun's shape is variable. Readers of these pages will know that we do not consider that the fact of this variation is proved, but the author of the paper in the 'Transactions' accepts Dr. Poor's conclusions with all confidence.

STEREOSCOPIC ASTRONOMY.—The instrument called the stereo-comparator, designed by Dr. Max Wolf, has been already described in these pages. At a recent meeting of the Academy of Sciences, M. Lœwy announced a discovery made by this astronomer by this method. There is a star which was already known to have a large proper motion, and in two photographs of its neighbourhood, separated by an interval of only four years, viewed stereoscopically, this star was distinguished from those around. In another case, two plates of a region in Leo, separated by fourteen years, were examined in the same way, and the proper motion of a star of the ninth magnitude was at once apparent. Moreover, Prof. Wolf says he was able to at once measure the proper motion with a precision above that of ordinary micrometric measures.

A stereoscopic combination of two enlargements of lunar photographs taken at the Paris Observatory shows in a striking manner, says M. Lœwy, the difference of level of the lunar formations. Comet Perrine, 1902 *b*, and the nebulæ of Orion and Andromeda present an interesting appearance under similar conditions.

SOSIGENES.—Mr. Lynn writes to us:—"In my letter to you dated April 2, and published in your May number, I referred to an earlier Sosigenes, a Stoic philosopher, as having flourished in the second century after Christ. The word 'after' was a slip for 'before.' I do not regret what has produced an interesting letter from Dr. Dreyer, but it is as well to point out that it was a mere inadvertence."

ERRATA.—P. 193, second line from bottom, *for* "Uranus and Neptune" *read* "Iapetus and Hyperion"; p. 196, line 21, *for* "before" *read* "after."

THE death is announced of M. Raphael Bischoffsheim, a generous benefactor to French astronomy. He provided the great equatorial for the National Observatory, Paris, contributed largely to the Pic du Midi and Montsouris Observatories, and established the fine observatory at Nice, of which General Bassot is now Director, and where so many recent cometary discoveries have been made. M. Bischoffscheim was an Honorary Member of the Paris Academy of Sciences.

THE Cambridge Philosophical Society has awarded the Hopkins Prize for the period 1897–1900 to S. S. Hough, M.A., F.R.S., Chief Assistant at the Royal Observatory, Cape of Good Hope, for his papers on the Dynamical Theory of the Tides, published in the 'Philosophical Transactions of the Royal Society.'

THE programme of Lectures to be given to University Extension Students during their visit to Cambridge in August next comprises several on astronomical subjects, the lecturers being Rev. T. E. R. Phillips, Mr. A. R. Hinks, Mr. Arthur Berry, and Mr. A. W. Clayden.

A COMMITTEE has been formed with the object of establishing a memorial of the late Sir William Wharton, K.C.B., F.R.S., Hydrographer to the Navy, who died at Cape Town in September last.

THE Senate of the University of Dublin has decided to confer the honorary degree of Sc.D. on Mr. E. T. Whittaker, F.R.S., Royal Astronomer of Ireland, on July 31 next.

THE Rev. Prof. J. G. Hagen, S.J., Director of the Georgetown College Observatory, Washington, D.C., has been appointed Director of the Vatican Observatory, Rome.

THE death is announced of Dr. F. M. Karlinski, formerly Director of the Observatory and Professor in the University of Cracow.

DR. E. ANDING, Professor in the University of Munich, has been appointed Director of the Ducal Observatory in Gotha.

FROM AN OXFORD NOTE-BOOK.

THE brief but able and suggestive statement by Mr. W. W. Bryant on the working of the International Catalogue Scheme, in the 'Monthly Notices' for March 1905, might have been expected to evoke some comment, but none, to the best of my knowledge, has been forthcoming. The five years for which the scheme was tentatively undertaken is nearly completed, and we do not seem to know much more than we did of the probable value of it. The inference seems to be that only after a considerable time will its value become apparent. Personally, I must confess that I have not, up to the present, had occasion to use the Catalogue; but there is always the hope of using it tomorrow. For instance, if one had an article to write for the 'Encyclopædia Britannica' it might come in handy; or for a text-book. Probably it will be consulted more frequently by the writers of text-books, and summaries, and histories than by those actually working at a subject; but if it turns out really useful to such writers, the work will not be thrown away. The most cheerful remark in Mr. Bryant's paper, to my mind, is that at the end, where he points out that some references are so inaccessible that even the indexer has to take a journey to find them, and suggests "the question as to whether it is worth while indexing papers so difficult to unearth." Many of us would answer emphatically "No!" from the point of view of the worker, for whom the Catalogue was designed. But if it is after all to be used, not by the worker, but by the historian, what then? Perhaps even the historian would ultimately gain by a decision to omit such papers. It would gradually become necessary to see that anything worthy of permanent record was recorded in some easily accessible place.

A DISTINGUISHED Oxford scholar has written six poems on "Pioneers of Light":—Copernicus, Tycho, Kepler, Galileo, Newton: the fourth poem being devoted jointly to the first three great names. The result has only been circulated privately, and the copy before me is labelled No. 26 only. But I think a wider publicity is deserved. Tentatively, I reproduce the most ambitious experiment. It certainly needs a courageous man to attempt the serious versification of Kepler's Laws; and the author has practically acknowledged failure to mitigate the third of them after all. But I am sure his enterprise will at least evoke admiring sympathy.

A fervid soul, as his own planet Mars!
  In body frail as flame, and sore distraught
  With toil: swifter than lightning in his thought,
Braver than Moslem in fanatic wars.

Despite the penury, which chills and bars
  Leading and light, he struggled through, he wrought
  Thesis on thesis, till the thing he sought
He found, and legislated for the stars!

The planets move, each in its own ellipse,
  So runs of three the primal truth sublime!
In equal times the radius vector slips
  O'er equal areas, runs the second rime:
The third portends my poetry's eclipse!
  The cube o' the distance is as the square o' the time.

IT was simple malice on the part of the calculators of ephemerides to announce a total eclipse of the Sun on August 30 last, when the visit of the British Association to South Africa had been all arranged. Many of us who were thereby prevented from going to the Cape are determined to look into this matter to see whether these eclipses cannot be predicted at least a few years ahead, so that those who plan distant visits for scientific men may be spared the disappointment of having their would-be guests called elsewhere at the last moment. Meanwhile we are grateful to Mr. Hinks for telling us so much of what happened. It is no easy matter to write a series of chapters such as that with which he has delighted us in this Magazine for some months back. When actually on the march, with incident crowding upon the attention, it is only too easy to persuade oneself that the proper time to write it all down is later on; and on return home it is equally easy to find so much requiring to be done that the record must be again deferred. But Mr. Hinks has paid no heed to these whisperings of the tempter, and there in black and white is a record of a notable journey, full of little things which we are all glad to know and should not have known. There are also somewhat mysterious references, capable of expansion; and we may hope that they will be expanded in due course. It is satisfactory to think that there is something in reserve. South Africa is full of wonders, as is indicated in the following extract from *The Tribune* for June 18:—

> SIR WILLIAM BUTLER'S CORRESPONDENCE.
>
> *Special to* The Tribune.
>
> II.—THE STRANGENESS OF SOUTH AFRICA.
>
> Johannesburg.
>
> "There is always something strange out of Africa" we are told. But it is only in our time that the full "strangeness of Africa" is apparent. And it is in the Southern portion of the Continent that the tree of strangeness attains its fullest growth. The Sun and his shadows go in wrong directions. He is away in the north; they seek the south. The Moon is stranger still. She lights her evening lamp at the off side of her face to that which she first Illumines for the benefit of Europeans. The winds are all strange, too. The breath from the north is hot—that of the south cold. The east blesses and the west blights. Rain—when it falls at all—falls mostly in summer. Many of the rivers never reach the sea, and the sea only gets into the river across shifting sand-bars, which make them hopeless to commerce.

ON p. 458 of the December number of this Magazine, Mr. Hinks referred to "the CLOCK, whose fame is already dear to everyone in Cape Town who knows the Royal Observatory." It is apparently not only the fame of the clock which is dear, judging from the following extract from the *Daily News* of February 28. The paragraph comes from a column article on the "deficiencies, gratuities, and losses" in the Navy Estimates, and the expenditure on the clock is placed alongside the gratuity of £8000 to Rear-Admiral Scott "for his inventions in connection with naval gunnery." Of course the expenditure and the gratuity must be judged by results; they may be the means of securing steps in advance worth *n* times the particular sums mentioned. But it is open to doubt whether the method adopted for obtaining the clock is likely to make it easier for other people to get money from the Government for equally worthy objects. If other estimates were to be exceeded in the way indicated, how long would our scientific work go on?

> ABOUT A CLOCK.
>
> Then there is a correspondence as to a claim presented by the Cambridge Scientific Instrument Company, Limited, for £991 11*s.* 6*d.* on account of an astronomical clock and accessories manufactured by them to the order of Sir David Gill, his Majesty's Astronomer at the Cape of Good Hope. Sir David had paid £250, leaving £741 11*s.* 6*d.* outstanding, in addition to £38 18*s.* 11*d.* for packing, etc., or £780 10*s.* 5*d.* in all. Sir David had acted upon an estimate for £150, and believed that the accessories would not exceed £100. The result was a proposal to offer the company £563 18*s.* 11*d.*, and "my Lords" to impress on Sir David that greater care should have been exercised by him in ascertaining the financial effect of the orders entrusted by him to the company.

THE years roll on and we make no adequate attempt to extend the circulation of this Magazine; and as regards myself I will frankly confess that the reason is simply that I did not know how to do it. But a circular received the other day from a kind correspondent put me to shame; I saw in a flash that it was what ought to have been said by the Editors, or by some one on their behalf, years ago, with the alteration of a few words only to suit the special circumstances. The alterations are easily made, and here is the result; and if the Editors do not shortly double their circulation the fault will clearly not lie with them:—

> We have the pleasure to inform You that, with the new year, our Magazine will be sent to the subscribers at the price of —— per year only.
>
> This is no little sacrifice; if one considers the great expenses at which we are put to; but we were pleased however to meet it, with the hope of increasing the number of studious men interested in our work, and of strengthening the energies of our Magazine, which profits in its moral and economical wellbeing from the sympathy and the help of the subscribers.
>
> Thus it has existed already 29 years, through good and bad times, but always serene and comforted by its ideality: the decorum of the Nation and the advantage of the astronomical conditions.
>
> With these sentiments and these hopes we apply to the friends of our studies and earnestly beg them to help us; and we should be glad of your subscription, as this is the best way to sustain and to elevate our aims and our enterprise.
>
> We should be more grateful to You, if You in thinking of the utility of our work, will exert yourself as a token of amiable solidity to make our desires known among good friends of science, for the progress of our astronomical studies.

A PATIENT lecturer wrote recently:—"Here is a sample of the teachableness of some people. I had mentioned Precession and 25,000 years, and a man asked afterwards whether that had anything to do with the 25,000 miles in the circumference of the Earth; so I emphatically denied any connection. But I was told later that he was heard to mutter 'I don't care what he says; I'll be bound it *has*!'"

IN the 'Life of H. Hawkins,' vol. ii. p. 124, there is the following story about the naming of a mare "Morning Star." The conversation is opened by the owner. "*Morning Star*," he said again, wishing me to ask why. "Now do you know *why* I call her Morning Star?" I answered truthfully that I did not, nor do I believe I should ever have guessed it. "Why," he said, with a merry laugh, "because she's *a roarer*."

FROM the *Queenstown Daily Representative and Express*, Jan. 9, 1906; the peroration to a long letter on "The Old and the New Astronomy":—

> Such is the base of our astronomy to-day, which is good enough for navigation and railways, but which in other respects, like our geometry, is faulty and irrational; as useful, mayhap, as astral photography, and the finding whether or not the stars are fit places for human habitation. Meanwhile, we must go back to our much slighted and criticised Old Testament for our chronology, and our scientific times and seasons. Neither is the year itself a uniform quantity. It is not 365 days, as we are told. It should be 366¼ day units, for sidereal years, like the radius, the chord of 60 degrees, and the circumference of the circle. A. McGILLIWIE.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXIX. AUGUST, 1906. No. 373.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1906 June 20.

A. C. D. CROMMELIN, B.A., *President*, in the Chair.

Secretary: J. A. HARDCASTLE.

Mr. J. A. Hardcastle read the Minutes of the previous Meeting, which were confirmed and signed.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of two new Members was unanimously confirmed.

The President then read the list of names proposed by the Council for the Council of the Association next Session. No further names being proposed, the President declared the list closed. He then appointed the following gentlemen to act as Scrutineers of the Ballot for the next Council:—Messrs. E. Holmes, A. Hutt, and A. R. Wesley.

Mr. D. W. Horner proposed the election of Messrs. Geo. Avenell and F. D. C. Baly as Auditors of the Accounts for the Session 1905–6; this was seconded by *Mr. E. W. Johnson*, and carried.

Mr. E. Holmes read a paper on "Stereograms of the Moon." The writer discussed various attempts that had been made to obtain stereograms of the Moon, and pointed out the causes of the unsatisfactory results, and suggested that diurnal libration would be sufficient, and that if a pair of smaller pictures were taken a more beautiful representation of our luminary might be obtained than any he had hitherto seen.

Mr. Saunder asked Mr. Holmes if he had seen the result of an experiment with two photographs of the Full Moon to which he had alluded.

Mr. Holmes said that the Member he had referred to had not been able to get a suitable night. The experiment was dependent upon the Moon being full and the night clear throughout.

The President said he was very glad the stereoscope was coming well to the fore in popularizing astronomy. They had lately, at the Royal Astronomical Society, Mr. Heath's stereograms of the stellar heavens, showing the stars standing in relief at their different distances. The stereogram of the Moon that Mr. Holmes had handed round was a good deal better than many he had seen. It was perfectly true that the relief was greatly exaggerated in Mr. Holmes's picture, and that it looked more like an egg than a globe, and Mr. Holmes's suggestion of using a smaller magnification and a smaller range of libration was a very good one.

The Rev. T. E. R. Phillips read a paper entitled "Observations of Mars in 1905." This was illustrated by several slides, and Mr. Phillips also showed a photograph taken by Mr. Offord, a Member of the Jupiter Section, who had succeeded in getting the principal belts and the S. tropical disturbance on the central meridian. On the original even the white spots preceding and following the disturbance could be seen. A 9¼-inch silvered-glass equatorial reflector was used with powers ranging usually from 217 to 342 diameters. Higher magnification was found impracticable, as, owing to the bad atmospheric conditions prevailing, the telescopic images were usually very tremulous and unsatisfactory. The *Maria* were, on the whole, not as well seen as could have been wished, partly on account of the tilt of the planet's axis, which caused the markings on the S. hemisphere, where most of the maria are situated, to be considerably foreshortened, and partly by the rarity of sufficiently good images. The conditions of observation were all against a systematic and critical study of the "canals." Those seen were comparatively few in number, and almost all of them appeared as soft hazy lines or streaks. No canals were seen really double, but the *Nilokeras*, as at sundry previous apparitions, was apparently composed of two separate streaks inclined to one another at a small angle and meeting at *Lacus Lunæ*. Only a few of the larger and more conspicuous "lakes" were detected. In conclusion, Mr. Phillips referred to the remarkable success of Mr. Lampland, of the Lowell Observatory, in photographing, in May of last year, a number of the principal canals, and held that the photographs of the canals proved at least that, however much they might question whether the lines were continuous or discontinuous, the principal ones were really there on the surface of the planet.

The President said it was rather unfortunate that at the times when the planet Mars came nearest to us, the most favourable conditions as regarded distance were not favourable as regarded the altitude of the planet in this country. The nearest opposition of all would occur next year, in July or August, but on that occasion the planet would be very far south, so that it would be very difficult to make good observations. In spite of a similar unfavourable altitude and weather conditions, Mr. Phillips had succeeded in making some very interesting and beautiful drawings. He was glad to hear that Mr. Phillips considered that the Lampland photographs settled the question of the objective reality of the canals. He (the President) took the same view, it might be remembered, in his Presidential Address last October, and it seemed to him clear that the photographs could hardly be an illusion as to the existence of some approximately linear markings there.

Mr. Goodacre read a paper, communicated by *Mr. H. G. Tompkins*, an Indian Member, on "Bright Rays on the Moon." Mr. Tompkins, arguing from phenomena that he had observed in India, suggested that the bright rays were deposits of sodium: the only condition necessary being that the Moon at some time in its existence possessed, like the Earth, air, water, and composition. Mr. Goodacre remarked that Mr. Tompkins had devoted a considerable amount of time to the endeavour to map some of the bright rays on the Moon. There was a short note from him in the last Report of the Lunar Section dealing with that subject, and since then he had continued to give his attention to these mysterious objects, the present paper being the result of his further investigations. Mr. Tompkins had also sent him a note saying that in the Punjab there were evidences of deposits of salt, which, if seen at a distance from the Earth's surface, would probably resemble some bright ray systems on the Moon.

Mr. D. W. Horner said Mr. Tomkins seemed very anxious to "pickle" the Moon, but if anyone who had seen it through the telescope could conceive that the beautiful markings seen could be salt and remain in the same state year after year for hundreds of years, it passed his (the speaker's) comprehension.

Mr. Saunder thought all present would wish to express appreciation of the paper Mr. Tomkins had sent. It was not a hurried paper. He had been making a great number of observations, extending over a number of years, which really did seem as if they might throw some light on one of the most puzzling phenomena of the Moon. Of course, the supposition that these rays were due to crystalline material was not new—it was a very old one; but what was new was the way Mr. Tomkins supposed this crystalline material had been deposited. Mr. Tomkins certainly assumed a great deal too much water on the Moon. It was not generally realized what a very small portion of the common atmosphere the Moon must have taken away with it if the Moon separated from the Earth in the manner suggested and almost proved by Sir George Darwin. Mr. Tomkins wrote as if it were generally recognized that there were signs of aqueous action on the Moon. But all geologists who had studied the Moon were agreed that there was no sign of aqueous action.

The President said they were much indebted to Mr. Saunder for his interesting criticism of Mr. Tomkins's paper. He (Mr. Tomkins) had for the first time suggested a terrestrial analogy to the bright rays on the Moon, and had shown that the analogy was a fairly close one.

Mr. Lynn read a paper on "The Total Eclipse of next January." In concluding he said that he did not know whether any astronomers from Europe or America proposed to attempt to observe the eclipse at Samarkand or its vicinity, but he had just heard that a lady, daughter of a late astronomer whose name was familiar to them all, hoped to repair thither for that purpose.

The President believed that Prof. Todd was thinking of going to view that eclipse.

Mr. Saunder having taken the Chair, the *President* contributed a note on the Planetoid TG, illustrated by a number of slides.

Mr. Hardcastle read a paper on "A Greenwich Mean-Time Sundial."

The President said the improvement which Mr. Hardcastle had introduced into the sundial of enabling anyone to read the mean time straight away was certainly a great convenience. This was the first time he (the President) had seen an actual picture of such a dial.

The Meeting then adjourned.

The Planets and Planetary Observation.

[Continued from p. 283.]

On a really good night, with splendid seeing, high powers can be utilized to a surprising degree, but, except in these rare cases the experience is that abnormal magnifiers afford no decided advantage. I have invariably found that when the power exceeds 500, single lenses are preferable, as giving more light and better definition, though the field is a very contracted one. Huygenian eyepieces of about 750, 1000, and 1250 can be very seldom applied to advantage, and when compared with views afforded by single lenses of similar powers a marked superiority always rests with the latter. Atmospheric conditions gave beautifully steady, sharp definition on 1905 November 6, and I tried two Huygenian eyepieces of about 710 on my 12½-inch Calver of 9½ feet focus. The view was fairly good and I thought it possible to discern a little more detail on Jupiter than with 200. With a power of 1210 the image became indistinct, details blurred, and general effect very unsatisfactory. With 912 single lens the disc was bright, well defined, and details remarkably distinct. Increasing this power to 1540 by inserting a Barlow lens, the image remained fairly good and all the chief markings were clearly traceable, including equatorial spots and many irregularities in the great S. equatorial belt &c. But nothing was gained over powers from 200 to 440

Rapid motion across the field, and the difficulty of keeping the object central, destroyed any possible advantage which might have accrued from the large disc. The result of many experiments proves that very high powers may suit special cases and be occasionally found of utility, but that moderate powers are decidedly preferable in general planetary observation. By "moderate" I mean eyepieces of about 150 to 250 on a 10-inch; 250 to 350 would be pretty high, while 350 to 500 may be considered the extreme magnification likely to prove successful under the very best conditions of seeing. "Pretty high" powers are often really necessary on Mars and Saturn, as their images are much smaller and bear magnifying better than the far more expansive disc of Jupiter.

With regard to the best time for planetary observation, it is obvious that the greater the altitude of the object the better will it be presented. It will rarely appear well defined within an hour and half of its rising or setting. Venus, Mars, and Jupiter, when they are sufficiently high in the heavens, can be splendidly seen from about an hour before sunset to about half an hour after. Saturn looks best at about the period of sunset and during the hour following. In the dusk of early evening planetary details often come out with surprising distinctness and markings can be glimpsed which are overpowered in the glare at a later hour, especially when a large telescope is employed. It is the same when we look at the Moon with the naked eye. In strong twilight we notice her dusky "seas" standing out with singular boldness, while later on, when she shines high and clear in the dark sky, we may scarcely distinguish any features amid the excessive brilliancy. I have often been privileged to enjoy magnificent views of the planets at about the period of sunset. It is not difficult without an equatorial mount to sweep up an object when its position is approximately known. A comet eyepiece with large field will soon enable it to be picked up, and when it has once been found in the daytime and its place relatively to some terrestrial objects below carefully noted, its detection on subsequent days becomes easy. The writer finds it practicable to bring Jupiter into the field in a very few minutes in this way and has taken hundreds of transits of the spots in sunshine. As a rule, however, observations are not of much utility if made more than two hours before sunset, as the details are then usually too faint and indefinite to be exactly located. On all such occasions the telescope must be shielded from the direct rays of the Sun, or currents of heated air will be set up in the interior of the tube and nothing will be satisfactorily seen.

The ordinary telescopic study of the planets consists of two main lines of work, viz., recording and delineating the aspects, dimensions, tones, tints, and positions of the markings and determining the times of mid-transit of all the irregular features with a view to deriving their rotation-periods. Of course a micrometer and clock-driven equatorial are great helps to accuracy in this work generally, but something may be done with an ordinary alt-azimuth stand alone. The observer may, by careful estimations, obtain the approximate dimensions and positions, and efforts should always be made in this direction, though the results will not have the same value as those secured with the micrometer. The length of an object in longitude may be found by noting the transit times of the preceding and following end and making proper allowance for its latitude. This is, however, chiefly effective in the case of Jupiter, whose rapid motion and large disc enable pretty exact deductions to be drawn even by estimation, and whenever the transits are numerous a good mean may be deduced.

The observation of the form, size, place &c. of the surface-markings on the planets is important, as assisting us to arrive at just conclusions with regard to their physical condition and phenomena. Changes of any and every kind should be noted, and it should be the aim to keep each individual feature under observation for as long a period as possible, so that its duration as well as its variation may be ascertained. Efforts should, in fact, be made to identify objects from one opposition to another; this refers particularly to markings on Jupiter and Saturn. It is satisfactory to note that the great red spot and south temperate disturbance on the former planet are being followed from year to year with interesting results. The case of Mars is different; here the markings seem permanent and subject only to occasional, temporary changes due to the interference of vapours. Martian features are probably much the same today as they were when Hooke, Huyghens, and Cassini surveyed the planet about two and a half centuries ago. The markings appear, in fact, to represent stable formations on the actual globe, whereas the objects distinguished on other planets, such as Venus, Jupiter, and Saturn, are certainly atmospheric and liable to all the vagaries of motion and aspect which mere cloud-forms are capable of exhibiting. Perhaps the statement requires some qualification as regards several very persistent features displayed on Jupiter, but that they are not real surface objects is sufficiently proved by their great individual differences in velocity.

The importance of following objects as long as possible, and fully tracing out alterations of shape, position, &c., appears to have only been appreciated in late years. Little more than a generation ago, when Lassell, Dawes, De La Rue, and other very able observers examined the planets, the work appears to have been done in a rather desultory way and very important objects were recognized only to be allowed to escape that close and repeated surveillance which could alone have furnished their life-histories and given later observers much valuable material for comparison.

With regard to taking transit-times, the question as to the best method has quite recently been thoroughly ventilated in the 'Monthly Notices' (vol. lxv. 1904–5). There is no doubt that simple eye estimation is practically as effective as micrometer measurement. An object near the central meridian of Jupiter becomes so rapidly displaced by the motion of rotation that transits can generally be obtained with a probable error of less than 2 minutes. And when a large number of such observations extending over a lengthly period are combined the errors of estimation are virtually eliminated; in fact their influence upon the computed rotation-period will scarcely exceed the small fraction of a second. The best efforts with the micrometer can attain no higher degree of precision. There seems to be a consensus of opinion on this point: Prof. Barnard, Major Molesworth, Rev. Phillips, Mr. Williams, and others have given expression to similar views. Prof. Hough, as a professional astronomer, feels a pardonable pride in the accuracy of micrometer work, but, while respecting the opinion he has formed after so much experience, there are some of us who materially differ from him in thinking that the micrometer is capable of giving results far superior to eye estimates.

If the rotation-periods of Jupiter based on instrumental measures were all put aside our knowledge of the velocities of the various Jovian currents would not suffer in the least. For the great bulk of observations consists of eye estimates and they have afforded data as reliable as they are exact; this has been amply proved by numerous comparisons and discussions in late years.

A source from which many doubts and errors have originated in determining rotation periods is to be found in the mis-identification of objects. Spots on Jupiter (and perhaps occasionally on Saturn) are so plentiful that unless the greatest care is exercised and transits are taken at short intervals it is difficult to be absolutely certain as to their identification. And mistakes in this respect are serious and may amount to many seconds. This point is an important one and requires far more attention than any question as to the method of taking transits. There have usually been 25 to 30 prominent white and dark spots alternating on the equatorial side of the south equatorial belt of Jupiter in late years; in 1903 there were a considerable number of spots in the N. temperate region of Saturn and individual objects exhibited great differences of motion. Certain of the markings in the same or nearly same longitude on the former planet appeared to blend or pass each other. These facts show the great need of assuring ourselves that the evidence of identification shall be amply sufficient in all cases. A month's interval between observations of a spot in similar longitude with many others must naturally give rise to serious doubts as to its identity. To avoid such complications, the necessity has long been recognized of making observations at short intervals and to accumulate as large a number of transits as possible. In the case of a special object, such as the red spot on Jupiter, there is no real urgency for oft-repeated observation except for the purpose of tracing minor variations of motion. Its appearance is unique and it may be identified at a glance, though one may not have seen it for years!

Locality is often an important factor in its influences upon planetary observation. The majority of people who use telescopes reside in towns or suburban districts, where manufactories emit volumes of disturbing and obscuring vapour, and where artificial light is apt to obliterate very faint objects. But in its effect upon definition, I think the noxious air of crowded localities has been much exaggerated. My observations are made from the northern side of Bristol, from whence a planet has usually to be viewed over several miles of chimney-pots, but complaint has seldom to be made of the bad seeing. Really unfavourable nights are commonly due to the general condition of the atmosphere rather than to the discharge of local impurities. When telescopic images are flaring and moulding as seen from here, they are usually behaving in similar fashion at the residence of an astronomical friend a few miles distant. The east wind is no respecter of places, and when it blows the observer may just as well relinquish his task. For the best results, no doubt a pure country situation is desirable, and those who have a good sea horizon may esteem themselves highly favoured. Still it is quite possible to work successfully under marked disadvantages of position ; it is incumbent on those who are placed in adverse circumstances, to be ever on the alert for favourable opportunities, and utilize them thoroughly whenever they occur. Wind does not always impair definition, though it is invariably troublesome in causing vibration and prohibiting the use of high powers. Occasionally planetary discs appear very sharp and distinct with half a gale blowing, but, unless the telescope is shielded from its effects, nothing useful can be done at such times. It is well known that the best nights are usually calm and mild with a little fog, and perhaps some partially transparent, thin, white cloud in the sky. It is then that the lustre of the brighter planets is moderated in a suitable manner to allow the more delicate features to show with great distinctness on the steady images. But, unfortunately, hours giving a near approach to perfection are comparatively rare. A person may obtain on one or two superlative nights the view of a wealth of detail clearly outlined on Mars, Jupiter, or Saturn, but may be quite unable to observe the same effect at any other time during the opposition. There may be many good nights—some exceedingly good,—but they will not quite match the excellence of the special occasion on which so much has been revealed.

On 1902, July 7, and 1903, July 9, definition was magnificent, and on each of these occasions I saw details on Jupiter which I failed to recover on any other nights of the two oppositions respectively. On 1903, May 21, in the beautifully steady air which prevailed, Mars exhibited a number of singularly distinct lineaments which could not be traced nearly so well on any second occasion during the year referred to.

Anyone who makes comparisons can hardly fail to be struck with the remarkable increase in the number of planetary observers about 30 or 40 years ago, and the far greater amount of work accomplished in the last generation than during any similar interval in the past. The wider interest displayed in this branch can be traced to several circumstances. The silver on glass Newtonian reflector was introduced about 1863, forming a very effective instrument obtainable at moderate cost. With's mirrors, mounted by Browning, became very popular, to be soon supplemented by the clever work of George Calver in the same line. Marth computed ephemerides for physical observation of Mars and Jupiter, and they were published in the 'Ast. Register,' and later in the 'Monthly Notices,' until his death in August 1897, when they were continued by Mr. Crommelin, to be ultimately transferred to the 'Nautical Almanac,' 1907.

The great Red Spot on Jupiter came under general observation in 1878, '79, and '80. The canals of Mars were seen by Schiaparelli in 1877 and 1879. Astronomical journals increased and became a means of disseminating news, as well as a convenient medium in which observational results might be reported. Thus there were *The Astronomical Register* (1863), *English Mechanic* (1865), *Nature* (1869), *The Observatory* (1877), *Sidereal Messenger* (1881, now *Popular Astronomy*), *L'Astronomie* (1882), and others. Several astronomical societies were founded, and contributed to make the science more popular generally. To these various influences the great development in the study of planetary surface-markings in recent years can be directly assigned. That it has considerably extended our acquaintance with these features, both as regards their appearances and motions, is certain, and there is every prospect of the research being maintained with increasing ardour and success in future years. The "canals" of Mars now form an old story. After standing on debatable ground for some years, their existence has been fully vindicated, and their delicate, complicated aspect and prolific distribution offer a rich prospect for renewed observations. The red spot on Jupiter is now no novelty, its intensely red colour has long since paled, and present observers may only trace the veriest shade of its former prominence, but it is still a familiar feature which continues to receive attention from every veteran user of a telescope. It may assume a striking aspect at any time, and repeat the changeable phenomena recorded of its prototype, and possible progenitor, visible in the days of Cassini and Maraldi two centuries ago. Saturn's equatorial white spot led to one of the observational successes of a generation ago (1876–7), and intimated the occasional occurrence of such outbreaks as that which so conspicuously affected the planet's N. temperate region in 1903. Great disturbances of this kind, interrupting the smooth aspect of the belts and zones, certainly take place both on Jupiter and Saturn, and they are known to be very frequently visible on the former orb, owing, perhaps, to the greater ease with which they may be detected.

The useful ephemerides initiated by Marth have proved of great benefit to amateurs, who have found them afford a ready means of computing the longitudes and determining rotation-periods of all the irregular features recorded; and the indebtedness of planetary observers to him for removing some of the difficulties incidental to their work forms only one example among many in which amateurs have had to thank their professional brethren for valuable assistance rendered. Many of the latter, in the few hours of leisure at their disposal, still find opportunity to direct, cultivate, and encourage amateur effort; and of this we need only refer to the case of the British Astronomical Association, and to the valued labours of Mr. Maunder, Mr. Crommelin, and others in connection with it.

[To be continued.]

Colours and Magnitudes of Double Stars.

There are two relations between the colours and magnitudes of binary stars which are usually accepted, viz.:—

1. When the magnitudes of the components differ but little, their colours differ little.
2. When there is a considerable range in the magnitudes, the difference in colour is very marked.

I remember seeing, some years ago, about 50 binaries separated into two classes, *i.e.* equal and unequal pairs, with the view of substantiating these statements*. Of course these would be binaries in the restricted sense where undoubted orbital motion is recognized.

One of the investigations not included in the recent discussion of the Struve Stars related to this subject. The result was put aside, but as I now think that it may have some bearing on the table of "Masses and colours" it is given herewith.

It is to be understood that *binary* is expanded into the more comprehensive *physical pair.*

Physical pairs in the 'Mensuræ Micrometricæ.'

Number of pairs.	Colour of Principal.	Colour of Companion.	Mean difference of magnitude.
20	YW	YW	$0^{m}.5$
185	W	W	0 .6
63	Y	Y	0 ·7
10	W	Y	0 ·8
14	W	BW	1 ·1
11	Y	W	1 ·9
60	W	B	2 ·2
21	YW	B	2 ·4
106	Y	B	2 ·5

* Was it Mr. Gore?

It will be seen that these not only show the statements to be true, but go some way towards a more general law.

These are mostly in the northern hemisphere. From 50 stars in the southern hemisphere and not in the 'Mensuræ Micrometricæ' the result is:—

White and White	0·5 mag.
Yellow and Yellow	0·6 „
White and Blue	2·9 „
Yellow and Blue	2·6 „

In sufficient agreement with the more complete discussion.

The distribution in right ascension of the principal colours is interesting. The numbers are:—

R.A.	W & W.	Y & Y.	W & B.	Y & B.	Total.
0h to 6h	34	9	20	37	100
6 to 12	42	13	17	26	97
12 to 18	46	17	18	27	108
18 to 24	61	24	19	38	142

1906, July 16. THOMAS LEWIS.

The Sun-spots of 1906.—I.

IT has been felt by several observers of sun-spot spectra that it would be a considerable advantage to them in their work if they were able to identify the groups of spots which they have had under examination by means of the Greenwich reference number, without being compelled to wait for something like six months after the end of the current year; that being the earliest time at which the Greenwich Results can be published. In order to meet this desire the Astronomer Royal has sanctioned my trying the experiment of making a preliminary examination of the solar photographs as soon as the necessary supplemental negatives have come to hand from India, and of numbering the groups from such rough preliminary examination, without waiting until the tedious process of measurement and reduction has been carried out. The accompanying Table is the first attempt at this numeration in advance, and covers the spots of the first three months of the present year. I hope to follow it by a similar table for the next three months in the next number of the 'Observatory,' and afterwards to give the numbers month by month, so that the July sun-spots will appear in the October number and so on, if this plan proves acceptable to the Editors.

It must of course be perfectly understood that though it is proposed to make the numeration of the groups definitive, not to be altered later, the other particulars relating to the groups are derived only from a rough-and-ready preliminary examination of the photographs and must not be regarded in any other light. It is quite likely that examination in the microscope will show many of the groups to have begun earlier or to have lasted later than they are here shown; but as minute markings are not suitable for spectroscopic examination, such corrections will not affect the usefulness of the table for the purpose for which it is designed. In like manner some of the spots marked as "Intermittent" may prove to be really traceable on some of the days whereon they now seem to be wanting. In the case of these "intermittent" groups, the numbers in the "Duration" column indicate the number of days upon which the group has been recognized. Thus Group No. 5779 was only seen on January 14 and on January 18, and therefore its duration is given as 2 days, not as 5.

Spot-Groups in 1906, January to March.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1905.	1906.	days.	d	°	°	
5765	Dec. 24	Jan. 3	11	Dec. 30·0	308	−15	Small stream.
6	26	6	12	Jan. 1·2	280	−10	Fine normal stream.
7	29	3	6	Dec. 29·1	320	+12	Stream.
	1906.						
5768	Jan. 1	2	2	Dec. 27·6	340	+ 7	Very small.
9	3	9	7	Jan. 8·8	179	+ 9	A regular spot.
5770	5	10	6	5·1	228	+10	Few small spots.
1	5	5	1	11·0	150	+11	Very small spot.
2	6	8	3	10·3	160	+ 6	Compact little cluster.
3	8	8	1	7·8	192	+ 7	Small spot.
4	8	15	8	13·8	114	+12	Small stream.
5	8	20	13	14·5	104	+ 7	**Return.** Large spot.
6	12	14	3	10·1	162	+12	Few small spots.
7	12	17	5	15·3	94	+12	Small. Intermittent.
8	13	18	6	18·5	52	+17	**Return.** Regular.
9	14	18	2	14·7	101	+11	Small. Intermittent.
5780	17	18	2	12·8	127	−14	Small spots.
1	18	18	1	22·1	5	+11	Very small spot.
2	20	20	1	23·3	349	+15	Small spot.
3	20	29	10	24·9	327	+12	**Return.** Stream.
4	20	20	1	18·8	48	+ 5	Small spot.
5	21	30	10	24·5	332	−13	Very fine stream.
5	21	24	4	25·7	317	+ 5	Few small spots.
7	21	29	9	26·4	308	−12	Few small spots.
8	22	Feb. 2	12	28·0	286	− 7	**Return.** Regular.
9	23	Jan. 23	1	21·4	13	−18	Few small spots.
5790	23	Feb. 1	10	26·9	301	+15	Straggling stream.
1	26	5	11	Feb. 1·1	233	+18	Large irregular group.

Spot-Groups (*continued*).

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	d	°	°	
5792	Jan. 26	Jan. 26	1	Jan. 22·0	5	−23	Small spot.
3	27	27	1	26·7	304	− 9	Small spot.
4	31	Feb. 4	5	30·3	257	+ 8	Pair of small spots.
5	Feb. 1	1	1	31·8	236	+23	Few small spots.
6	2	2	1	31·9	235	− 9	Small spots.
7	4	4	1	Feb. 6·9	156	−16	Very small.
8	4	15	12	10·6	107	+ 9	**Return.** Regular.
9	6	6	1	3·7	198	−29	Small spots.
5800	6	7	2	10·9	104	− 7	Small spots.
1	11	15	3	10·8	105	+13	Small. Intermittent.
2	14	26	13	20·3	339	−10	**Return.** Regular.
3	17	28	12	22·5	310	+14	Regular spot.
4	18	18	1	21·6	323	− 4	Few small spots.
5	19	20	2	18·9	358	− 6	Few small spots.
6	19	28	10	23·4	299	+17	Irregular stream.
7	21	26	6	22·8	307	−14	Unstable scattered group.
8	24	24	1	20·4	339	+22	Very small spots.
9	27	Mar. 2	4	24·7	282	− 8	Small cluster.
5810	28	3	4	26·2	262	+13	Stream.
1	28	Feb. 28	1	28·8	228	+ 8	Very small spots.
2	28	Mar. 2	3	Mar. 3·7	190	+15	Few small spots.
3	28	Feb. 28	1	5·8	162	+15	Very small spots.
4	Mar. 2	Mar. 7	4	6·7	150	+20	Small. Intermittent.
5	2	10	9	7·6	138	−15	Regular.
6	6	18	13	12·4	75	+ 6	Large regular spot.
7	10	16	7	10·0	106	+20	Compact stream.
8	10	15	6	11·4	88	− 6	Stream. Much change.
9	11	11	1	8·6	125	+18	Very small spots.
5820	15	19	5	14·2	51	+23	Stream.
1	16	27	12	21·7	313	+16	**Return.** Very large.
2	18	27	10	23·0	295	− 9	Stream. Much change.
3	19	24	6	18·8	351	−30	Stream. Much change.
4	19	27	9	21·3	317	−16	Stream. Much change.
5	19	25	7	22·0	308	−19	Stream. Much change.
6	19	30	12	24·5	276	+14	Very large double spot.
7	19	20	2	24·9	270	+ 9	Small spot.
8	19	27	8	25·5	262	+16	Regular. Intermittent.
9	20	27	8	22·7	299	−13	Stream. Much change.
5830	21	Apr. 2	13	26·8	245	− 9	Regular spot
1	22	Mar. 25	4	27·3	238	−11	Small cluster.
2	25	27	3	23·7	286	− 8	Very small spot.
3	25	25	1	23·8	286	+12	Very small spots.
4	25	Apr. 1	8	29·1	215	−17	Stream.
5	26	Mar. 27	2	24·7	273	−12	Very small spots.
6	27	Apr. 1	6	27·0	242	+ 8	Small. Intermittent.

The "Date for C. M." is simply the mean longitude of the group in another form; it gives the date when the mean longitude of the group reached the central meridian, whether the group was active at that date or not. Thus Group 5768 is given as being on the central meridian on December 27, though it did not form until January 1, and Group 5772 as being on the central meridian on January 10, though it had disappeared by January 9.

The longitudes and latitudes are intended to represent the mean places of each entire group during the whole period of observation, and will suffice for the purpose of identification, but they are estimations only, not measures; the indications of area, also, are merely rough eye estimations.

Notes on the Principal Spot-Groups.

Group 5766.—A fine stream of the usual type; *i. e.* two large spots with a line of small spots between them. The leader was a large regular spot, area about 300-millionths of the Sun's visible hemisphere. A large, complicated, dark structure, with little or no umbra, had formed close behind the leader by January 1. The group was a new one.

Group 5769.—A regular spot, closing up.

Group 5775.—Return of Group 5747. It ran its first course, from 1905, December 12 to December 24, as a large regular spot followed by a short train of small spots. Its general appearance was now much the same as in December, but it was diminishing; say area about 300 instead of 450.

Group 5778.—A regular spot closing up. The return of Group 5762, which ran its first course from 1905, December 22 to December 28.

Group 5783.—Return of Group 5767. It ran its first course from 1905, December 29 to 1906, January 3, as a stream of small spots of no remarkable size. In this second apparition the stream attained a fair development by January 24, but no great spots formed either as leader or rearguard. The leader, a small regular spot, alone remained after January 26.

Group 5785.—A very fine stream of area about 1300. It consisted chiefly of two very large elliptical spots, crossed by many bridges.

Group 5787.—Revival of Group 5765. A few small, unstable, and diminishing spots.

Group 5788.—Return of Group 5766. A regular spot, slowly closing up.

Group 5791.—A striking group, consisting of a number of spots scattered over a wide area, and not drawn out into a stream. Two large principal spots. Area about 700.

Group 5798.—Return of Group 5775. Third appearance. At first it was a regular spot, slowly closing up, but a sudden outburst had taken place immediately to the south of the spot by February 9. This was not a revival of the activity of the group, as it seemed rather to hasten its disappearance.

Group 5802.—Return of Group 5785. A regular spot, area about 130, slowly closing up.

Groups 5803 and 5806.—These two groups might be considered as forming together a single, very long, straggling stream; the return, or rather the revival, of Group 5790. Group 5803 consisted chiefly of a large, regular spot, area about 180, followed by a short train of small spots. Group 5806 was an irregular stream of small spots.

Group 5816.—A large regular spot, mean area about 250, closing up. Occasionally some small companions.

Group 5817.—A compact stream forming suddenly. Area about 200.

March 18.—A period of great activity set in lasting until the end of the month.

Group 5821.—Return, or rather new outbreak, of Group 5803. A number of spots, none very large, scattered over a wide area. The spots increased and filled up the entire area of the group, forming an enormous complex spot, area about 1400, with but little umbra.

Group 5826.—Very fine pair of regular spots. The two spots coalesced and formed a single well-developed spot, area about 700.

Group 5828.—A considerable regular spot, following Group 5826. It broke up rapidly.

Group 5830.—A well-defined regular spot, area about 100, very slowly closing up.

Groups 5818, 5820, 5822, 5823, 5824, 5825, and 5829 were all groups undergoing rapid changes, but they were short-lived, and no large spots formed in any one of them.

I shall be obliged if observers who are likely to make use of these tables would kindly let me know if the foregoing form appears likely to meet their requirements, and I shall be glad to consider any suggestions that may tend to make it more useful.

E. Walter Maunder.

The Variation of Latitude.

In August 1903 the appearance of the first volume of results of the Variation of Latitude campaign was signalized in this Magazine by a short history of the whole subject up to the end of the year 1902. Of the second volume*, which has just appeared, there seems little to say here, as it consists mainly of tables of figures similar to those of the first volume, explanation of which may be found in the number above quoted. This second volume gives the results of the work during the years 1902, 1903, and 1904, which may be summarized by saying that in 1902 the pole of the Earth moved about its mean position in a curve approximating to a circle of 0″·15 radius. In 1903 the circle was somewhat larger, the distance of the pole from its mean position coming to a maximum of about 0″·30 in June of that year. In 1904 the spiral curve began to turn inwards and ended at the beginning of 1905, at a distance of 0″·15 from the mean position.

But between the appearance of these two volumes several minor memoirs on the subject have been published. An excellent summary of these by M. R. Radau appeared in the *Bulletin Astronomique* for May last, which we take leave to translate.

M. Albrecht published in two notes the provisional results of observations in the years 1903 and 1904, which have been discussed by MM. Wanach and Förster. In 1903 the amplitude of variation appeared to have reached its maximum, for in 1904 it decreased a little. The variation of latitude is represented by the formula:

$$x \cos l + y \sin l + z,$$

where the annual term z is included, independent of the longitude λ, which was discovered by M. Kimura. To obtain an idea of the nature of this term, the following table, which gives its value for every tenth of a year, may be examined:—

	1900.	1901.	1902.	1903.	1904.
	″	″	″	″	″
0 ..	+0·028	+0·062	+0·025	+0·036	+0·059
1 ..	+0·021	+0·055	+0·013	+0·046	+0·056
2 ..	+0·008	+0·022	−0·003	+0·029	+0·027
3 ..	−0·014	−0·005	−0·024	−0·013	0·000
4 ..	−0·029	−0·026	−0·043	−0·012	−0·014
5 ..	−0·033	−0·036	−0·053	−0·029	−0·015
6 ..	−0·025	−0·032	−0·046	−0·022	−0·007
7 ..	−0·008	−0·016	−0·022	−0·005	+0·001
8 ..	+0·019	+0·007	+0·008	+0·022	+0·019
9 ..	+0·047	+0·025	+0·023	+0·043	+0·047

It can be seen that it is a question of hundredths of seconds, and it is instructive to note that the mean values of latitudes themselves vary from one year to another by quantities of the same order, as the following table shows:—

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* 'Resultate des Internationalen Breitendienstes.' Band II. Von Th. Albrecht und B. Wanach. Berlin, 1906.

Mean Annual Values of Latitude (39° 8′+).

	1900–01.	1902.	1903.	1904.
	″	″	″	″
Mizusawa	3·62	3·64	3·62	3·62
Tchardjui	10·67	10·66	10·69	10·66
Carloforte	8·93	8·93	8·91	8·94
Gaithersburg....	13·20	13·21	13·18	13·14
Cincinnati	19·31	19·33	19·34	19·36
Ukiah	12·07	12·04	12·06	12·07

There are then discordances of 5 or 6 hundredths of a second between the annual means, and a term with an annual period of which the amplitude is often less than these discordances.

The Japan station of Mizusawa excels all the others in the quality of the observations, judging from their theoretical weight. It may be added that the Observatory of Tokio furnishes each year a contingent of observations, which agree perfectly with those of Mizusawa. The Observatory of Leyden continues to note the variations of latitude.

From the beginning of 1903, M. Kimura has undertaken at Mizusawa, with his assistant M. Nakano, a more extended series of observations than the international scheme required. The discussion of this material showed that there existed no systematic difference between results drawn from the observations of two groups of stars observed each night and those deduced from four groups. A subsidiary result of the same series is that it furnished proof that there was no diurnal variation of latitude, or, at any rate, no appreciable variation (*Ast. Nach.* 4040–41).

In two previous notes (*Ast. Nach.* 3932, 3981) M. Kimura considered the period of the principal term of the variation, which is supposed to be about 14 months, and which he brought to 14½. He established, in fact, that the curves plainly indicate a period of six years, which ought to correspond to five periods of the principal term; the value of the period sought would then be 12 years = 438 days. A more precise consideration has shown that the cycle of 6 years is really 5·98, which gives for the period in question 436 or 437 days, instead of 428 or 430.

Prof. H. G. van de Sande Bakhuyzen in a very interesting article (*Ast. Nach.* No. 3937) passes in review the hypotheses which have been put forward to explain the origin of the enigmatic term discovered by Kimura. It is known that Chandler has shown that this term can be explained if we admit a mean stellar parallax of 0″·13, but this parallax appears to be much larger than one has a right to suppose. Is it possible to invoke an annual oscillation of the centre of gravity of the terrestrial globe? Adopting for the term in question (z) an amplitude of 0″·04, the periodic displacement of the centre of gravity ought to have an amplitude of 1·5 metres; the total displacement would be 3 metres. This might be attributed to the melting of the polar ice, but it is necessary to suppose that the melting removes each year 3 million cubic kilometres of ice, that is a layer one kilometre thick, covering a polar cap of nine degrees radius. The heat that the Sun sends us would not be sufficient to produce this effect. It is true that one can find other causes for an oscillation of this kind. Such a one has been indicated by M. R. Schumann (*Ast. Nach.* 3877). It may also be asked if all these variations are real or if they are due, at least in part, to instrumental errors or to the influence of anomalies of refraction.

The observations which have been made at Leyden since 1899 with a Wanschaff zenith-telescope may be profitably examined to elucidate this question. Attempt has been made to determine the value of z for the three years 1900, 1901, and 1902. But the numbers that Prof. Bakhuyzen has found at Leyden have little resemblance to those that Prof. Albrecht gives for the whole of the international stations; the amplitudes and especially the phases are very different. But this discordance, since it happens in measures so delicate and which nearly touch the limits of observation, is in truth what might be expected. Perfect accordance would leave us sceptical.

Among the instrumental errors which cause apparent variation of latitude, M. Kimura has noted those which arise from the influence of temperature upon the micrometers (*Ast. Journal*, vol. xxiii.). But the Leyden observations do not appear to show the existence of errors from this source.

There remain the anomalies of refraction caused by variable inclination of layers of equal density. This inclination is certainly subject to slight local variations according to the state of the atmosphere. There are also troubles which result from inequality of temperatures outside and inside the observing-room, as the researches of MM. Faye, Bakhuyzen, Nyrén, and Schumann have shown, a question which was discussed in the Annual Report of the Geodetic Institute (1893–1894, p. 7). But how can it be supposed that these variations are the same for the six stations situated in climates so diverse? It seems, on the other hand, that the judicious installation of these stations disposes of anomalies of this kind.

There is, we think, pressing need of undertaking methodical observations for a simultaneous study of astronomical refraction and of the state of the upper layers of the atmosphere, as Admiral Mouchez proposed to make at the Paris Observatory in 1882 by the aid of a captive balloon. The researches that eminent astronomers have recently effected to determine, in a more exact manner, the constants which enter into the theoretical expression for the refraction would have given us complete information on this question if care had previously been taken to find out the distribution of temperature and pressure in the plane of the meridian up to a certain height. It would have been sufficient for this to have set up registering apparatus.

Another explanation of the Kimura term lately proposed must be mentioned. M. Courvoisier, taking up an idea already given out by M. Schaeberle, has noted that the existence of a material centre or of a condensation of the ether in circumsolar regions would involve an annual refraction analogous to the annual parallax of the fixed stars, when the point opposite to the Sun would play the part of the zenith (*A. N.* 3990, 3991). Now these refractions would tend to diminish the parallax. One would have, then, to admit that the mean parallax of $0''\cdot13$, which would be necessary to account for the Kimura term, should be reduced by annual refraction to $0''\cdot02$, the apparent value for the parallax of the latitude stars according to Kapteyn. It remains, then, to show, by other and more direct proofs, the reality of the annual refraction of the fixed stars, and this is what M. Courvoisier has endeavoured to do. His conclusions have been contested by M. Pannekœk, and a discussion has ensued which has much shaken the new hypothesis (*A. N.* 4008, 4012, 4024, 4031; also Note by M. Harzer on Cosmic Refraction, *A. N.* 4025).

In a very short note (*A. N.* 3981) M. W. de Sitter has again drawn attention to a small term which would result from the luni-solar attraction and which, combining with the term supplied by a parallax of $0''\cdot02$, would give an amplitude of $0''\cdot17$, representing about half the Kimura term.

The questions involved in the interesting problem of variation of latitude are many and complex and are not near solution. It is to be hoped that the new international latitude stations that have been decided on will furnish their share of observations which will help to clear up this obscure point. Two of these stations, situated on the parallel of $-31^\circ\cdot55$, should have begun work in 1906 January. These are Bayswater in Western Australia (long. $7^h\ 44^m$ E.), and d'Oncatwo in the Argentine (long. $4^h\ 15^m$ W.), the one entrusted to Mr. Curt Hessen, the other to Signor Luigi Carnera.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Sir Jonas Moore.

Gentlemen,—

It was greatly due to Sir Jonas Moore that the Royal Observatory was established at the time and place that it was. He urged its foundation, superintended its building, and recommended Flamsteed to the notice of the king. A mural tablet at the entrance to the Octagon Room, which practically composed the original Observatory, commemorates his superintendence of its construction. With this all who have frequently visited the establishment are familiar; but mistakes seem to have been made with regard to the official title of Sir Jonas, of which only the initials (R.T.S.G.) are given on the tablet. That title in full is "Rei Tormentariæ Supervisor Generalis," *i. e.* "Master-General of the Ordnance," and the last two words are here in the ablative case, in concord with "curante Iona Moore," so that, written in full, it is "Rei Tormentariæ Supervisore Generali." In Mr. Webster's recent excellent little book on Greenwich Park this is incorrectly printed as "Rex Tormentaricæ Supervisare Generali," and in the sixth volume of 'Old and New London' (p. 217) the inscription is given, but the initials representing Moore's official title are omitted.

It has occurred to me that the epitaph placed in the Tower Chapel in memory of Sir Jonas might be interesting to your readers. It is given in the *Gentleman's Magazine* for July 1817 (vol. lxxxvii. p. 3), having been sent to the Editor by Mr. E. L. Swift, then Keeper of the Regalia, in consequence of some doubts which had been raised as to the dates of the birth and death of Sir Jonas, and runs thus :—

M.S.
Jonæ Mori equitis aurati, in agro Lancastriensi
apud vicum Whiteloe * nati VIII° die Februarii
anno à Partu Virginis MDCXVII°.
Qui, ob egregiam erga Principem suam fidelitatem,
summam in rebus Mathematicis scientiam,
et singularem in negotiis peragendis
solertiam et industriam,
a rege Carolo Secundo ad officium Supervisoris generalis
rei tormentariæ bellicæ evocatus est.
Qui, munere dum dignè fungitur,
ingenuas etiam disciplinas, artesque mechanicas,
non magis ad animi sui oblectamentum,
quam publica patriæ commoda,
studiosissime excoluit.
Et imprimis Astronomiæ et Nauticæ Artis fautorem
munificentissimum se præbuit,
easque promovendi causâ
Speculum Grenovicensem (jubente rege)
extrui curavit,
instrumentis idoneis locupletavit,
editisque Mathematicis operib. utilissimis
orbi inclaruit.
Vixit annos LXII devixit XXVIII° Augusti, a° Christi MDCLXXIX.

The remainder of the inscription states that his only son, who held the same office and honour as his father, was buried in the same tomb, and that the monument was placed by Mary, his daughter, in memory of her father and brother, her husband, William Hanway, taking part in it.

Sir Jonas was largely occupied during the Commonwealth in draining the fen districts. He was knighted by Charles II. after an expedition to Tangier in 1663, for the purpose of inspecting it with a view to its fortification. He wrote several books on arithmetic, geometry, and other parts of mathematics. That on arithmetic was first published in 1660, with a dedication to the Duke of York, afterwards James II.; it has as a frontispiece a portrait of the author taken at the age of 45.

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* Probably the place now called Whittle, near Preston.

His son, also Sir Jonas Moore, succeeded Lord Dartmouth as Master-General of the Ordnance, and it is probable that Captain Jonas Moore, who was killed at the seige of Carthagena in 1741, was *his* son and a grandson of Sir Jonas, who took so prominent a part in the foundation of the Greenwich Observatory.

Yours faithfully,

Blackheath, 1906, July 16. W. T. LYNN.

Ancient Eclipses.

GENTLEMEN,—

I think the recent discussions on ancient eclipses will have satisfied most of your readers as to the necessity of further research on the subject, and the research which is required is of a kind of which few persons are capable. We require, I apprehend:—

1. An exact statement of the terms or at least the import of the original records.
2. A determination on purely historical grounds of the limits in time and place which these records admit of. I say "on purely historical grounds," for we cannot admit dates which are themselves fixed (or supposed to be fixed) by the records of eclipses to be used as *data* for ascertaining the dates of early eclipses.
3. An investigation of the lapse of time between the eclipse and the record of it, with an inquiry into the probable means of knowledge of the person who records it and his general reputation for reliableness or the contrary.

With the best attainable results on these three points before him, an astronomer could either fix the date and place of the eclipse with much greater confidence than at present, or else declare that a decision between the claims of competing eclipses could rest on no stronger basis than conjecture.

For the investigations to which I refer, we require the assistance not of an astronomer but of a well-read historian. And considering the importance of *ascertained* eclipses in fixing historical dates, I think we need not despair of getting some able historian—Prof. Bury, for instance—to undertake the task. As an Irishman, I should wish to see Ireland foremost in the field, and I think if Prof. Bury investigated the historical records and then consulted with his colleague Sir Robert Ball as to the astronomical aspect of the question, the result might be a closer approximation to certainty as regards the dates of several ancient eclipses, and a rejection of others as hopelessly uncertain unless further information with regard to them should come to light hereafter.

I remain,

Truly yours,

Dublin, 1906, July 14. W. H. S. MONCK.

Archilochus and the Eclipse of B.C. 648.

GENTLEMEN,—

Mr. Nevill, in a paper which appears in the May number of the 'Monthly Notices of the R. A. S.,' says that I quoted Archilochus as referring to the taking of Magnesia by the Cimmerians. Now I think that almost * the only public reference I have made to that poet is in a letter printed in the 16th volume of the *Observatory* (p. 269), in which I merely remarked that his date may well have been somewhat later than that generally given, and that the solar eclipse described in one of the fragments which are all that remain of his works may have been that of the 6th of April, B.C. 648.

The fragment to which Mr. Nevill refers is, in Gaisford's edition of the 'Poetæ Minores Græci,' numbered lxxii. It was preserved by Strabo (lib. xiv.), and consists of only one line, formerly read

κλαίειν θάσσων, οὖ τὰ Μαγνήτων κακά.

But Gaisford's conjecture has been generally accepted that for *θάσσων* we should read *Θασιῶν*, and that Archilochus is calling upon us to bemoan the calamities of the Thasians rather than those of the Magnesians. Strabo himself comments that the passage proves that Archilochus was of later date than Callinus, who had spoken of Magnesia as being in a prosperous state and carrying on a successful war against Ephesus. He quotes a line of the latter, which speaks of the havoc wrought by a fierce army of Cimmerians, and thinks it refers to the taking of Sardis by them. The destruction of Magnesia by the Treres, a tribe of Cimmerians, occurred in later times, probably in that of Gyges, who applied for help to Assur-bani-pal, of Nineveh. However that be, it is clear that Archilochus lived as late as the time of Gyges, for he mentions that king in another fragment, numbered by Gaisford x., which is given by Plutarch in his 'De Tranquillitate Animi,' cap. x., thus:—

οὖ μοι τὰ Γύγεω τοῦ πολυχρύσου μέλει

(with three other lines), but he does not mention the author's name. This, however, had been given by Aristotle, in referring to the sentiment at the beginning, in 'De Arte Rhetorica,' lib. iii. cap. 17. The wealth, indeed, of Gyges seems, like that of his great grandson Crœsus afterwards became, to have passed into a proverb; and the 15th of the Odes which were formerly ascribed to Anacreon (though it is now understood by their dialectic forms that they are really by a much later writer) begins with a similar disclaimer of regard for his riches (probably imitated from Archilochus).

But Aristotle's quotation may be allowed to prove that the date of Archilochus was not earlier than that of Gyges, and that he was probably a contemporary of that king of Lydia. Now, as mentioned above, Gyges applied to Assur-bani-pal (who is known to have ascended the throne of Assyria on the death of Esarhaddon in the year corresponding to B.C. 668) for help against the Cimmerians. This was not in the early part of the reign of Assur-bani-pal, which was taken up with a war against Elam. Hence it may be inferred that some of the poems of Archilochus were written several years later than B.C. 668, and that there is nothing in history to lead us to reject the view of Prof. Millosevich and Mr. Cowell that the eclipse described by him as total may have been that of B.C. 648 (= −647) April 6, observed at Thasos.

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* I say "almost," because I practically repeated this in the 16th volume (p. 150) of the Journal of the British Astronomical Association.

Yours faithfully,

Blackheath, 1906, July 2. W. T. LYNN.

Chinese Records of Sun-spots.

GENTLEMEN,—

In his interesting note on Chinese records of sun-spots, published in the *Observatory*, no. 370, Prof. A. Schuster expresses the hope that some one may be able to say whether there were any Chinese records of sun-spots previous to those tabulated by Prof. Hirayama.

May I take the liberty of referring Prof. Schuster to a list which was published in the *Bulletin Astronomique* of Paris in February 1904, where he will find two records of an earlier date?

One, A.D. 20, I consider as doubtful. But there is no reason for questioning the other, dated 28 B.C. And it is certainly remarkable that it falls within the pentade 70–74, which is considered by Prof. Schuster as one of maximum.

The list of Prof. Hirayama is unfortunately not at my disposal for comparison, but it evidently does not coincide exactly with mine. Supplementing one list by the other would probably supply a more complete and trustworthy table.

Possibly some remarks in my list may lead to considering a few records as doubtful. In several cases, the Chinese have evidently mistaken for a sun-spot what was a mere optic phenomenon. The observation of December 807 is a typical example; there was only a parhelion.

My list contains fewer sun-spots, but if treated in the same way as that of Prof. Hirayama, is at least as much in favour of a periodicity. The secondary maximum of the fifth pentade is smoothed down, though it must be confessed that the figure for the eighteenth pentade is undesirably large.

I have the honour to be, Gentlemen,

Yours sincerely,

Zi-ka-Wei, China,
1906, June 15.

J. DE MOIDREY.

OBSERVATORIES.

PARIS.—M. Lœwy begins his report for the year 1905 by saying that the work of the Observatory was seriously disturbed during that period by various events. The death of M. Paul Henry, which happened at the beginning of the year, naturally had an adverse effect on the astrographic work. The energies of five of the staff were absorbed practically for two-thirds of the year by work pertaining to the solar eclipse, and two others were frequently absent for different reasons.

The report is essentially one of promise. The following researches have been begun, or are in contemplation, but there are yet no definite results which can be quoted. M. Bigourdan is beginning a determination of the constant of aberration by M. Lœwy's method *, using the equatorial of the West Tower. M. Hamy is arranging to make spectroscopic observations of the radial velocity of stars, and for this work M. Gautier is making a spectroscope which will be attached to the Grand Equatorial coudé. As this arrangement will enable the spectroscope to be kept in a fixed position during use, there is little limit to its size or to the arrangements for keeping it at a uniform temperature. For this reason it has been possible to make a spectroscope of great dispersion.

M. Hamy will also make researches on the displacements of the solar atmosphere by new methods. These are not explained in the report, but it seems that a Rowland grating is an essential. M. Nordmann is measuring the strength of the ionization of the atmosphere by an apparatus designed by himself which he also used during the solar eclipse of August, and the same astronomer is undertaking a research on colour photometry of variable stars.

But besides telling of these things of the future M. Lœwy has to report that several valuable publications have been prepared. The second volume of the Astrographic Catalogue, containing the measured rectangular co-ordinates of 70,093 stars in the zone two degrees broad whose centre is at declination 23° N., has been prepared, also the ninth part of the splendid Photographic Atlas of the Moon; and with reference to this the report contains some valuable opinions on lunar matters which may be translated as follows:—

> Study of these photographs of the Moon has shown on the lunar surface undoubted evidence of very different periods separated by long ages. The great mass of facts gathered leads us to follow up this synthetic history rather than to take up the discussion of ancient theories. Among these doctrines there is one, however, which deserves examination, as recent publications have put it forward. There are some who consider the surface of our satellite as formed—deformed rather—by numerous and large projectiles. In the introduction to this Part IX. we have undertaken to discuss the different admissible

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* *Ibid.* vol. xxviii. p. 426.

origins of these hypothetical assailants and to confront the markings really registered on the photographs with the possible consequences of such collisions. One can conceive three distinct sources for the bodies the shock of which would have given birth to the seas and craters of the Moon. Either they are due to swarms of meteors circulating around the Sun, or else they are hurled by terrestrial volcanoes, or, lastly, having been detached from the Earth when fluid, they have circulated around that body in orbits differing little from that of the Moon.

When one examines, by applying the laws of celestial mechanics, the course of events which would result from these hypotheses, one easily recognizes that the causes thus suggested are contradicted by the real facts. We have sought to show by calculation that these suggestions are erroneous. For this purpose, a table is supplied from which can be predicted the lot of an asteroid passing with varying degrees of velocity in the neighbourhood of the Moon or of the Earth.

These tables do not make it appear that the meteoric origin is *a priori* excluded, but they give a view of what would be the distribution of the impressions on the surface of the Moon and the relative velocity at the moment of shock. Whether the projectile had penetrated in the lunar crust, or whether it had skimmed over it, the resultant embossing would be in both cases absolutely different from that which the seas and other lunar formations show us. And it does not seem to us that another alternative can be offered.

The intervention of terrestrial volcanoes is not less in contradiction with the most sure and most generally admitted facts of geology. There remains calling for more attentive examination only the third theory—that of the annexation by the Moon of the whole of a ring of satellites of the Earth gravitating around it in the neighbourhood of the lunar orbit.

Here again the computed tables help as in predicting the conditions of the collision and show that the consequences are not in accordance with reality.

The Earth would have retained some of these satellites visible to its inhabitants. All the corpuscles susceptible of being annexed by the Moon would have fallen on her in a time relatively short, insufficient for the formation of a solid crust, more insufficient still to give it a great thickness quite recognisable by levels of different heights. Then the incidences upon this shell would have been, in the great majority of cases, very oblique and not normal as the observation shows.

Finally, our studies tend to disprove an external origin for the seas as for the craters and to assign the preponderatirg part in their formation to volcanic forces.

This work of publication and preparation for new researches has not hindered the progress of the meridian work. The Cercle méridien du jardin was reported last year to be in need of some further modifications. A principal fault seemed to be that when the instrument was turned the readings of the two circles on either side did not show the same difference of readings. The two circles, which where of the kind with spokes radiating from the centre, have been replaced by solid metal circles and the trouble appears to have been removed. The year, as to this instrument, was spent in determining screw-values and instrumentel constants. It now appears to be in a perfect condition, quite suited for the research on latitude and the positions of fundamental stars which it is proposed to pursue.

The astrographic work is now under the direction of M. Puiseux.

As this is being written, a report comes to hand of the death of M. Bossert, a valued member of the staff of this observatory.

KODAIKANAL. — The staff of this Observatory consists of Mr. Michie-Smith, the Director, four Assistants, a Photographic Assistant, and a Writer, who all appear to be natives. The post of Assistant-Director is unfilled *. This is essentially a solar physical observatory, and from the report of the Director for the year 1905 it appears to be for the most part in good going order, for photographs of the Sun, which are on the same scale as those of the Greenwich series, were taken on 327 days as against 264 in 1904. Observations of widened lines in sun-spot spectra were made on 179 days. These are made only when the spots are large enough to render the results satisfactory; on other days the neighbourhood of the spots is studied as regards the behaviour of the hydrogen and helium lines. The helium line D_3 proved particularly interesting.

Prominences were recorded visually on 297 days, on 250 of which the observations were considered satisfactory. There is a spectro-heliograph at Kodaikanal, but this does not appear to give as good results as could be wished. Mr. Michie-Smith states that troubles arise from the presence of a large amount of dust in the neighbourhood, which settles on the slits, and from want of steadiness in the image of the Sun, which arises from the unsatisfactory position of the building. Attempt is being made to remedy this by covering the rocky ground around the building with vegetation, but this will take some time.

The pamphlet includes the report of the Madras Observatory, which is now devoted solely to Time Service and Meteorology, and of which Mr. Michie-Smith is also Director.

STONYHURST. — The astronomical paragraphs in Father Sidgreaves's Report for 1905 contain a statement of the mean disk-areas of the spots on the Sun in 1905, compared with that of the five previous years, as shown by the drawings made at Stonyhurst, which run in order of date:—0·55, 0·29, 0·33, 1·93, 2·54, and 6·8. The increase in mean daily range of the declination magnet follows these figures somewhat closely. The stellar spectrograph has been employed on nearly every available night, but only 212 exposures have been made on 110 nights. Arrangements are being made to take an efficient part in the international programme of observations of sun-spot spectra.

NATAL.—We have received Mr. Nevill's report of the work of this Government Observatory for the year 1905, but as this is almost wholly of a meteorological and magnetic nature the report calls for little notice here. The transit-instrument appears to be used for time determination for the purpose of the time-signals, and the large equatorial was used to obtain a series of positions of Vesta, which have been communicated to the Royal Astronomical Society.

* [Since this was written, we hear that Mr. John Evershed, to whom we offer sincere congratulations, has been appointed to this position.—EDS.]

NOTES.

Comet Notes.—Finlay's Comet (1886 VII.), which is now making its third return, was observed on July 16 by Herr Kopff, of Heidelberg, being 6^{m} west, 1° south of the place given by M. Schulhof's ephemeris, given last month. A second ephemeris was computed by M. G. Fayet, from other elements by M. Schulhof, which, however, were known to be only approximate, before the above observation was made. The observation shows that this ephemeris, which was published in *A. N.* No. 4106, requires a correction $-12^{m}\ 11^{s}$ in R.A. and $-1^{\circ}\ 37'{\cdot}5$ in declination, and that the time of perihelion passage will be 1906, Sept. 8·35 Paris Mean Time. These corrections have been applied to form the following ephemeris:—

At Paris Midnight.

	R.A.	Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Aug. 1..	1 43 14	3 4·5 S.	Aug. 17 ..	3 57 33	9 18·5
5..	2 18 36	0 20·4 N.	21 ..	4 25 41	11 29·9
9..	2 53 27	3 39·4	25 ..	4 51 4	13 16·6
13..	3 26 41	6 41·0 N.	29 ..	5 13 54	14 41·9

The comet was described by Herr Kopff as "Bright." It has been photographed at Greenwich with the 30-inch reflector, with 20 minutes exposure, and the resulting image is not conspicuous.

In *A. N.* 4108, dated July 27, there is given a photographic observation by Herr Kopff on July 14. Also observations made since July 16 at the Collegio Romano, at Vienna, and at Utrecht. This last observation shows a correction to M. Fayet's ephemeris, $-12^{m}\ 58^{s}$ and $-1^{\circ}\ 51'$.

Minor Planet Notes.—Two new planets have been found: UM (mag. 13) by J. H. Metcalf, at Taunton, Mass., on June 14; UN (mag. 12) by Prof. Wolf, at Heidelberg, on June 21.

Obituary.—J. F. Bossert. With great regret we hear that the National Observatory, Paris, has suffered a further loss by the death of this valued member of the staff, which happened on June 22, last.

Joseph François Bossert was born in November 1851 and entered the Observatory in 1867. He was appointed *aide-astronome* in 1872 July, promoted to the rank of *astronome-adjoint* in 1879 January and *astronome-titulaire* in 1903 December. At the time of his death M. Bossert was superintendent of the computing-bureau, having for many years acted as deputy superintendent to M. Gaillot. M. Bossert's personal work beyond what might be considered his official duties was of the nature of calculation. In 1896 December he received the Valz Prize of the

Academy for a catalogue of proper motions of 2641 stars and for other work of cataloguing ancient observations of Lalande.

GEORGES RAYET. Another loss to French astronomy has been caused by the death, which happened on June 14, of M. G. Rayet, Founder and Director since its foundation of the Observatory of Bordeaux-Floirac. M. Rayet was 66½ years old at the time of his death. His successor in the Directorship will probably be M. Luc Picart, formerly assistant at the Bordeaux Observatory, now Professor of Astronomy at Lille.

PHOTOGRAPHIC TRANSIT-INSTRUMENTS.—The invention of an apparatus to record the time of transit of stars across the meridian should be in a fair way of being realized, if, indeed, the thing is not akin to "the philosopher's stone" or "the squaring of the circle," remembering the number of persons who seem to be working at it. Accounts of two plans, or rather of instruments which are being experimented with, have been lately published—one in the Annals of the Tokyo Observatory, vol. iii., the other, which refers to an instrument at the Paris Observatory, is in the number of the *Comptes Rendus* for July 16. As these instruments are of quite opposite types it may be of interest to contrast them. In the Paris apparatus the photographic plate is moved by mechanism akin to an equatorial telescope, and the star or stars are photographed on the plate with comparatively long exposure. The image of a *fixed* slit is photographed on the moving plate at different recorded instants. The slit represents the meridian, so that there results a photograph of stars with the meridian photographed on it at various places. Slight consideration will show that the reduction consists in (1) determining the errors of this meridian relative to the true meridian, (2) measuring the positions of stars relative to these lines representing this meridian at different times, and it is not difficult to see that factors to be considered are the rate of the driving-clock, the determination of the exact clock-times of exposure of the slit, and errors of measurement, besides the ordinary meridian errors, the chief of which, that is the level error, the experimenters at Paris appear to have determined by photographing the slit by reflection from mercury as well as directly on the same plate.

In Prof. Hirayama's method the plate remains stationary during a transit and the wires, or rather a reticule ruled on glass, is photographed thereon. The star-image travels across the plate behind an occulting bar, which is lifted, at each alternate second, for a second, say, by means of a circuit made and broken by contact-springs in the sidereal clock, and the star-trail appears as a line of dots . . at right angles to the transit-wires. The first step in the reduction here of course consists in measuring the dots with reference to the wires, and it is obvious that the beginning of each dot should be taken; but as it is difficult to exactly locate the edges of the dots, it has been found necessary to bisect the distance between the "make" and "break" and consider the point of bisection applicable to the middle point of the times of these. The apparatus has its limitations. With an exposure of one second fifth-magnitude stars gave a measurable image, but fainter stars could not record themselves. For stars of greater declination than 73° with exposure of one second, the successive impressions were not distinctly divided from one another.

Since the final object of all such pieces of apparatus is not merely to make transit observations, but to make very accurate transit observations which shall be free from all errors of the size of personal equation and variation of personal equation due to magnitude, it appears that the perfect photogaphic transit has not yet been evolved, although these are two very good attempts.

ZODIACAL LIGHT OR TWILIGHT.—Prof. Barnard, writing from Yerkes Observatory to the *Astrophysical Journal* (1906, March), says that though the Sun at the summer solstice is there 24° below the horizon at midnight, and twilight is supposed to end when it is 18° below, still, every summer that he has been there, for a week or two at midsummer he has seen a twilight glow passing along the north horizon for a couple of hours in the middle of the night. He says that he has watched this move from the west under the pole to the east, being apparently the evening twilight passing along the north horizon to join the dawn. Prof. Newcomb not long ago said that he had seen a phenomenon of somewhat similar kind from the top of a mountain in Switzerland, but he described it as a luminosity surrounding the Sun on all sides, which he considered to be the zodiacal light. Prof. Newcomb says that though there is no absolute proof that the light visible along the north horizon at midnight when the Sun is at a depression of 25° is not a form of twilight, the rapid diminution and disappearance of twilight when the Sun is somewhere between 15° and 18° below the horizon seems to show that this is the end of actual twilight.

AN ASTRONOMICAL BIBLIOGRAPHY.—A Committee of Bibliography and of Astronomical Sciences of the Royal Observatory of Belgium has undertaken to publish a list of the observatories and astronomers of the whole world. The Chairman of the Committee, Prof. P. Stroobant, will be glad if amateur astronomers, University Professors, or others not attached to any observatory but who are actively engaged in astronomical research, will send their names and addresses, with a description of the work on which they are employed, to him at the Royal Observatory of Belgium, Uccle, Belgium.

The Liverpool Astronomical Society.—The Rev. R. Killip has been the recipient of an address and cheque as acknowledgment of his services as Secretary of this Society, from which office he now retires. Mr. R. C. Johnson, who made the presentation, referred to the enthusiasm which Mr. Killip had awakened among the Members, and the encouragement he had given to the study of the science.

Large Sun-spots.—At the moment of writing, July 31, there are two spots on the Sun, both visible to the unaided eye. The preceding and larger spot, which passed the central meridian on July 30, is interesting, since it was first seen as a small spot on July 28, and has grown to its present size on the visible hemisphere. To-day it is about 10° in length, in breadth it extends over about 6° of solar latitude, and its area is nearly 2000-millionths of the visible disk. This spot is in latitude about 16° S.

A Fine Summer.—The records show that the total sunshine at Greenwich for the first seven months of this year is slightly in excess of any previous year since 1877, when a sunshine recorder was first installed. The previous greatest was in 1899; but it should be noted that in 1887 the Campbell recorder was replaced by the Campbell-Stokes, and the earlier instrument is considered to be less efficient.

The British Association.—The meeting this year will be held at York, from August 1 to 8. In the Department of Astronomy and Cosmical Physics a joint discussion has been arranged with the Geography Section on the necessity for the re-measurement of the British geodetic arc, which will be opened by Major Hills. Prof. H. H. Turner will probably make some remarks in the same Section on another subject. Some account of the proceedings will probably be given in the September number. It is announced that the University of Leeds, in connection with this meeting, will confer Honorary Degrees on several persons. Prof. Ray Lankester, this year's President of the Association, and Prof. H. H. Turner will, amongst others, receive the degree of Sc.D.

The Total Solar Eclipse of 1907 January 13–14.—Enquiries are already being made about the Eclipse of the Sun which will happen early in next year. A contribution to the 'American Journal of Science' for March last by Prof. David Todd and Mr. H. Baker gave in tabular form computed times of phases of the phenomenon for different places, which it is hoped to reproduce in a subsequent number. Meanwhile we quote the following:—

Six total eclipses of the Sun happen during the next six years.

Of these the tracks of those of 1908 and 1911 are wholly confined to the Pacific Ocean, with the possibility of observing stations on only two or three difficult islands; the eclipse of 1909 is too near the North Pole, and that of 1910 too near the South Pole for ready observation; totality of 1912 can perhaps be excellently obtained in Brazil; but of them all that of 1907 seems least uncertain to yield significant results.

Fortunately the track of the eclipse of 1907 January 12–14 is wholly on land. But a good part of the region visited is so remote and difficult of access in Mongolia and the Gobi desert that it can be occupied only by equipping tedious and expensive expeditions. Only one station in Mongolia, Tsairosu, seems likely to be considered.

But the western half of the track crosses Turkestan, a trans-Caspian region penetrated by the imperial railways of Russia. For Europeans a convenient route would be Berlin, Warsaw, Moscow, Samara, Orenburg, and Tashkent. On this railway twice and about two-thirds of the way from Tashkent to Samarkand, lies Jizak, only a few miles from the exact line of central eclipse. Other easily accessible places near Jizak and well within the belt of totality are Zaamin, Nau, and Ura-tiube. The last is practically central. If we go further east, the track of the eclipse leads to a region more and more difficult in every way, although the totality is a few seconds longer there, and the eclipsed Sun a few degrees higher.

The situation between the L. C. C. and the Royal Observatory has obtruded itself into contemporary literature in various ways, as will be seen from the last page of this number. It has even been done into Latin verse, as below, the causing circumstances being these: It is a custom at Westminster School on a certain occasion in each year for the boys to declaim original verses, conceived in a more or less humorous vein, on current events, and at "Election dinner" this year the following "epigram" among others was recited, the thesis being "Quieta non movere":—

Machina vim lucemque molit. Specula inde labascit,
  Fuligo, ut cœli palpitet aura, facit.
Devius astra rogat Phœbus, sibi qua sit eundum.
  Astra negant, ubi sint, scire tremente polo.
Quid latum, quid longum incerto examine pendet,
  Hora, dies illa est, quæ mora cuique placet.
Fulminis ex alto subducti fraude Promethei
  In cœlum ira diu dissimulata redit.

On July 4 the honorary degree of Sc.D. of Dublin University was conferred on Prof. E. T. Whittaker, F.R.S., Royal Astronomer of Ireland.

FROM AN OXFORD NOTE-BOOK.

THE threatened danger to Greenwich Observatory from the establishment of a generating station half-a-mile away has at least had one satisfactory result. It has revealed a widespread interest in the integrity of the Observatory, which is a little surprising and not a little welcome. The debate in the House of Lords, of which a full account was given in the last number of this magazine, well exemplifies this interest and sympathy: while an illustration of a different kind is reproduced, by permission of the Editors, at the end of these notes. It is true that some of our sympathizers know very little about us; they credit the Astronomer Royal with a beard, for instance, though no Astronomer Royal ever had a beard, if we may trust the evidence of their portraits given in the Frontispiece to vol. xxii. of this magazine. But the public is sufficiently in earnest to overlook such matters of detail, and we may hope that, with such powerful backing, serious dangers may be ultimately averted.

IT is some time since the Observatory had this kind of anxiety. The introduction of railways of course brought with it troubles of the kind, and Airy had a long fight to keep railways at a distance. But in 1872 he was able to write (see his 'Autobiography,' p. 294): "I trust that the contest, which has lasted thirty-seven years, is now terminated." At nearly the same interval subsequently, a new contest has arisen: we may all hope that its life may not be so long a one.

THERE were a great number of experiments made on the tremors produced by railways, not only at Greenwich but elsewhere.

The following extract from the 'Armagh Catalogue' (1859) will serve as an illustration; and it draws attention to an effect of vibration to which little attention has been paid. Yet the effect which is influenced by vibration—a relief of the instrument from strain after setting it—is probably far-reaching and important. Reasons were given a dozen years ago (see Mem. R. A. S. li. p. 121) for thinking that the R—D discordance was due to some phenomenon of the kind; and a series of flexure observations made soon afterwards (see the Introduction to the 'Greenwich Observations' for 1894) showed definitely that when the transit-circle telescope is turned through 180° there are changes amounting to $0''\cdot10$ and $0''\cdot20$ per minute of time in the positions of the circle and telescope. These are quantities deserving more extended measurement. But here is the extract from the 'Armagh Catalogue' (p. vii):—

> The terminus of the Ulster Railroad, which is the nearest part of that line, is 700 yards distant, about 50° W. of N. The tremors caused by its traffic produce various disturbances, which I have described in 'Proceedings of the

Royal Irish Academy,' vol. v. p. 287: the most important of which is the motion of the circle if a train passes *after* the bisection of a star, but *before* the microscopes are read. This arises from the friction at the pivots being released, in consequence of the jar caused by the vibration of the ground, and I have known it to amount to 4″. I have, therefore, laid down the rule to reject observations made under such circumstances; as, however, tremors are observable before the train is heard, and several minutes before it arrives at the station, it is possible that such errors may have occurred without our being aware.

Before leaving topics connected with Greenwich, may I offer a word of congratulation on the admirable series of places of the VI. and VII. Satellites of Jupiter published in the May number of the *Monthly Notices* and recently circulated in reprint? The VI. Satellite has been photographed on 36 nights and the VII. on 15: there are usually several plates on each night, and accurate places have been measured (and reduced to position-angle and distance) on all these plates: which involves the taking and measuring of supplementary plates to get the places of the faint comparison stars. This means an enormous amount of work which comes as an *addition* to the ordinary work of the observatory—already heavy enough; and says much for the energy as well as the skill of the staff. But a more astonishing thing still is to find that the faint VII. Satellite can actually be photographed at Greenwich in 17 minutes exposure! We need not be disturbed about the suitability of the Greenwich sky for astronomical work in the presence of facts like this. I am sure Prof. Perrine will forgive us for recalling again his expression of opinion (in October 1905 while passing through England) that it would not be found possible to photograph the satellite at Greenwich at all. The statement represents the best possible expert opinion in all respects save one: for no one could be better acquainted with the faintness of the satellite than the discoverer, nor will any one claim to know much more about the powers of a reflector than Prof. Perrine; only in the possibilities of the Greenwich climate was his experience scanty, and we feel sure that no one will be better pleased than he to find that he was mistaken in his estimate.

Being holiday-time I will venture to reproduce a holiday problem which seems to have some merits. How could a cricket-match be won before the losing side had bowled a ball? No astronomical reason for asking the question is advanced, though by the exercise of some ingenuity a link with scientific problems might be found in the notion of the concrete existence of negative quantities, or again in the common necessity for explaining a strange but observed fact. For the fact of a match being so won was actually observed by the late Provost of King's, and is recorded in his recently-published biography, in a sentence which will be found at the end of these notes, according to previous practice, in order to give those who like to solve problems unaided a chance to do so.

THE resignation of the Editorial Chair of *Punch* by Sir Francis Burnand in the early part of the year afforded an occasion for the publication of various reminiscences; and a kind correspondent sent me a column which appeared in the *Morning Leader* of March 26. There are at least two utterances in the column which may without incongruity be reproduced here. The first is brief and to the point:—

> *Happy Thought.*—. . . Never go anywhere without note-book.

The second is a piece of astronomical history, and may be given without further preliminaries:—

> *Among the Stars.*—How did it happen? Thus. A kind uncle of mine, George Bishop, had a house and grounds on the river side, at Twickenham, just opposite the Duke of Buccleuch's at Richmond. In these grounds George Bishop had built an observatory for the well-known astronomer, Mr. Hind. This was open to the Astronomer Royal night and day; he could come and make any observations he liked, no matter what their strength might be, as, unless my uncle happened to be present (and *he* didn't mind), it was not likely that there would ever be anyone to hear the astronomer's wise sayings, unless he happened to be accompanied by his clerk, an astronomer minor in the same line of celestial business as himself. Certainly "in their courses" the stars were in my favour.
>
> "You can have this room to yourself," said my uncle to me, "the next one Du Chaillu will occupy"—the eminent African explorer was a visitor there for some weeks,—"only," my uncle went on to say, "you'll both have to clear out whenever Hind wants to be here for his work."
>
> But the good astronomer was very considerate. Whenever he did arrive it was late at night, and he and his clerk watched till nearly dawn. Then by the time that either Du Chaillu or I were ready for action, the stargazers had vacated, and we were alone to do our worst, or best.
>
> Occasionally Du Chaillu would look in on me for a chat, or I would ask him if he were inclined for a ride, row on the river, or a walk. Sometimes, as it subsequently appeared, he had only "dropped in" for the purpose of reading to me extracts from his forthcoming book, when he would dramatically illustrate the scenes, with the aid of tables and chairs for rocky ground, and a walking-stick for a rifle.

READERS of 'Happy Thoughts' will remember that the astronomer is made use of in that immortal work to give local colour, though they may not have known who was the original. The paragraphs have been quoted in these notes—I was going to say recently, but on looking up the reference I find it is nearly ten years ago (April 1897). Perhaps they may be reprinted alongside the authentic version of the facts:—

> Being in need of quiet, in order to commence my great work on Typical Developments, I have found a charming retreat on the banks of the Thames, somewhere about Twickenham, or Teddington, or Richmond, or Kingston, and all that part. . . .

I discover for the first time that I've got a neighbour who looks at the Moon and Jupiter every night through a large telescope. He asks me would I like to step in and see Jupiter? . . . . I have stepped in and seen Jupiter (who gave me some difficulty in getting himself into focus) until my head aches. No writing to-night.

(Next night.) Neighbour over the hedge asks me "Would I like to have a look at Jupiter?" I say I won't trouble him, He says no trouble, just get the focus, and there you are. He *does* get the focus, and consequently there I am. I leave my 'Typical Developments,' Chap. 1. . . . . . Looking through the telescope makes one's head ache. We *did* have some brandy-and-water. Shan't stop up so late again. Cocks begin to crow here at midnight. It's quite light at midnight. . . . .

4th and 5th. Happy days. Disturbed in the evening by Jupiter, Saturn, and the Moon, which have always got something remarkable the matter with them.

FROM a serial story called "Nostalgia" in the *Fortnightly Review*, October 1905:—

Regina had once dreamed of an eclipse of the Sun. Reading Gabrie's page, she remembered that dream, because there was reproduced in her the same feeling of fearful darkness, of portentous silence, and terrible expectation.

For a moment. When the moment had passed she again saw the light of the Sun, felt again the vibration of life, perceived that everything in the outer world had retained its proper aspect and position, and that nothing was changed. . . .

FROM the *Telegraph* of Calcutta, concerning the rains of last autumn; reprinted in the *Daily News* of 1905 September 28:—

Storm and rain of unwonted severity passed over the metropolis at 3 P.M. on Tuesday last, cooling the city a great deal and bringing relief to suffering citizens. The heavy clouds swirled horribly over the city, the lightning forked out its myriad tongues, and heaven's cannon reverberated from tower to tower and flashed warlike fire at heaven's portals. Men in the street bustled on; the animals roared, crows and kites whistled, and gigantic trees, after rocking to and fro, came down in a mighty crash! The towers shook; palaces, cottages, and huts rumbled in a mighty chaos, and windows were blown bodily away from their sockets. Presently the rain descended in quivering sheets and the dust on the roads turned into mud, and the horses mired a dead deal. The torrents precipitated from the black clouds deluged the street, and dead dogs, cats, and crows ran down the currents like leaves and straws. Several umbrellas were borne along with the currents, which had evidently roamed truant from their legitimate owners. Stray bullocks and cows that had been reposing in the meadows squelched shoulder deep in the flood bellowing loudly for help. The rain was accompanied with very cool wind after the storm had subsided. Every pit by the larger roads was full to the brim presently, and traffic was little in evidence during the time the storm lasted. The spell of the heat is broken now; it had reached so acute a stage that even Europeans carried umbrellas and fans while walking in the streets, a sight which savoured of the lady. The city has been thoroughly washed, and plague is already at a low ebb.

Τέγγε πνεύμονας οἴνῳ· τὸ γὰρ ἄστρον περιτέλλεται
῞Α δ’ ὥρα χαλεπά, πάντα δὲ διψᾷ ὑπὸ καύματος.

Moisten your lungs with wine. The dog-star's sway
Returns, and all things thirst beneath his ray.

This verse of Alcæus, and the translation, with which Thomas

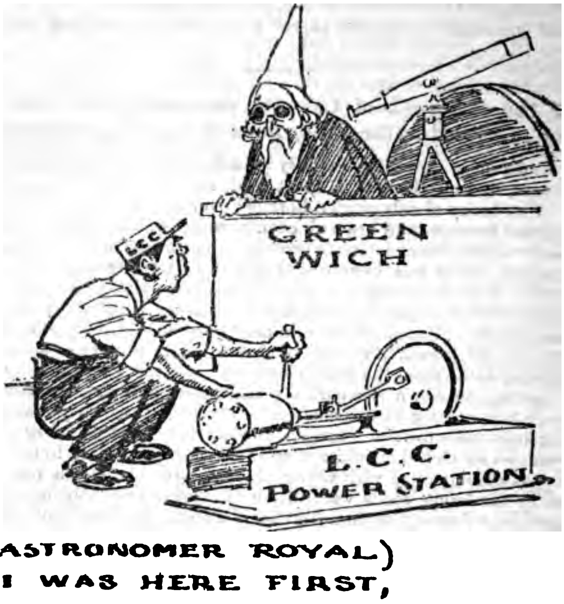
Love Peacock heads the 3rd chapter of 'Gryll Grange,' may serve also as an introduction to the following paragraph, which represents the result of researches by a *Star* meteorologist :—

> Dog-days are dog days because a bright particular star, known to stargazers as the Dog Star, makes friends with the Sun for that length of time. The star rises with the Sun, and adds to its heat.
>
> Dog-days end on 11 Aug., when presumably the Dog Star goes to the dogs.

Key to the cricket problem on a previous page :—

> A few good stories, one of which records how Austen Leigh—himself, as a young man, an enthusiastic cricketer—"was much impressed by seeing a match won before the losing side had bowled a ball. The King's Choristers played the Trinity boys. Trinity got out for 0, and the first ball their bowler attempted to deliver was a 'no ball'"!

The following cartoon appeared in a number of the *Illustrated Mail* of recent date :—



(ASTRONOMER ROYAL)
I WAS HERE FIRST,
I MEAN TIME.
YOU MEAN TROUBLE
YOU'VE UPSET LONDON
NOW YOU WANT TO
UPSET THE WORLD

No. 374.] SEPTEMBER, 1906.

SEP 29 1906

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXIX. SEPTEMBER, 1906. No. 374.

Meeting of the British Association at York, August 2 to 8, 1906.

The Address of the President, Prof. Ray Lankester, contained a little astronomy, since it was part of his plan to give a short account of the progress of all branches of science; but it is unnecessary to repeat this here, since the facts must be well known to our readers. In his introductory remarks to the Mathematical and Physical Section, the President (Dr. E. H. Griffiths) touched on the subject of the physical condition of the Earth and radio-activity:—

> Mr. Strutt has shown, arguing from known data, that the maximum temperature at the bottom of a crust of about forty-five miles in thickness must be in the neighbourhood of 1530° C., although some amount of uncertainty is necessarily induced by our want of knowledge of the conductivity of rock at high temperatures. Anyhow, it is probable that at the depth indicated the temperature does not exceed the melting-point of platinum. Such a crust would contain about one-thirtieth of the Earth's volume, and if throughout it the radium heat energy were of the average of that exhibited by many samples examined by Strutt, the temperature of the Earth could be maintained until our stores of uranium suffered sensible depletion. Such an assumption would lead to the conclusion that the whole of the central portion of the Earth consists of non-radio-active substances at an approximate uniform temperature somewhat below the melting-point of platinum. A brief summary of the evidence previously at our disposal may not be out of place.
>
> In the first edition (1867) of Thomson and Tait's 'Natural Philosophy' we find the tidal evidence summarised as follows: 'It seems certain, therefore, that the tidal effective rigidity of the Earth must be greater than that of glass.'
>
> In the 1883 edition of the same work a discussion of the question by Prof. George Darwin is given. He states: "On the whole we may fairly conclude, whilst there is some evidence of a tidal yielding of the Earth's mass, that yielding is certainly small, and that the effective rigidity is at least as great as steel."
>
> In a later paper * Darwin pointed out that this conclusion was based on the

---

* Proc. Roy. Soc. 1885.

assumption that oceanic tides would have their equilibrium value, and that the validity of this assumption was open to doubt. Nevertheless, the evidence clearly indicated a high degree of effective rigidity.

Hough * discussed the variation of latitude, and, after correcting a small mistake of Newcomb's (who was the first to suggest the explanation), found the prolongation of the Eulerian Nutation from 305 to 430 days as indicating an effective rigidity of the Earth about equal to that of steel. Wiechert, of Göttingen, found that the mean density, ellipticity, and precessional constant were consistent with the hypothesis of homogeneous core with lighter surface layer.

Mr. R. D. Oldham, in a paper on the "Propagation of Earthquake Waves," came to the conclusion that the evidence pointed to a central metallic core, and to the existence of marked differences in the physical constants of the core and the surrounding crust. He, however, assigned a comparatively small radius to this core, viz., about 0·55 that of the Earth.

I will now call your attention to the light thrown on this subject by the recent investigations of Prof. Milne. The difference in the rate of propagation of earthquake waves through the Earth's interior and through the crust has led him to the conclusion that the material below a depth approximating to thirty miles is of a uniform nature, and that the change in physical constitution is abrupt at some such depth as that indicated. He writes as follows: —

"For chords which lie within a depth of thirty miles the recorded speeds do not exceed those which we should expect for waves of compression in rocky material. This, therefore, is a maximum depth at which we should look for materials having similar physical properties to those we see on the Earth's surface; beneath this limit the materials of the outer part of this planet appear rapidly to merge into a fairly homogeneous nucleus with a high rigidity."

In the 'Transactions of the Royal Society' for 1905 will be found a paper by Lieut.-Col. S. G. Burrard on "The Intensity of the Force of Gravity in India." Colonel Burrard writes as follows:—"Geodetical observations have shown that the density of the Earth's crust is variable, but they have not given any positive indications of the depths to which these observed variations extend. All calculations of the depths of subterranean variations in density and of the mountain compensation have, therefore, to be based on arbitrary assumptions of depth. The fact that the plumb-line seems generally to respond readily to the results given by the pendulum perhaps justifies the inference that the observed variations in the density of the Earth's crust are not deep-seated. If an abnormal amount of matter exists in the crust near the surface, it will exercise direct effects upon plumb-lines and pendulums in the vicinity, but if it lies at a great depth its effects, especially on plumb-lines, will be less perceptible . . . I have taken several instances of abnormal pendulum results from table, and have found in each case direct response from the plumb-lines at neighbouring stations. This conformity could hardly ensue if the variations in density extended to greater depths than thirty or forty miles. Our results do not justify us in asserting that no deep-seated variations in density exist, but they do justify the belief that the variations in density which have been discovered are apparently superficial."

The Report on Seismological Investigations had some astronomical interest: first, because it compared the number of recorded earthquakes with Prof. Albrecht's variation of latitude curve, and it was sought to show that these were more frequent when the change in direction of pole movement was comparatively rapid than at other times. Secondly, this report contained the following paragraphs on the change of level on two sides of a valley, the connection of which with astronomy is evident:—

---

* Phil. Trans., A, 1895, 1896.

It has been found that under certain but frequently recurring conditions the two opposite sides of a valley move in opposite directions at the same time. On bright fine days the inclinations of the sides of a valley decrease. At night they increase. A valley may therefore be supposed to open and close. These conclusions, which do not necessarily apply to all valleys, are based on observations taken in two very different localities. The first were made in Tokyo, Japan, by means of horizontal pendulums giving continuous photographic records, installed on the two sides of a valley cut in alluvium.*

The second series were made on the two sides of a valley cut in chalk at Shide, near Newport, in the Isle of Wight. On the western side of the valley the instrument employed was an astronomical level reading to 1″·0 of arc. On the eastern side change of level was continuously recorded by a horizontal pendulum easily reading to 0″·5 of arc. Both were well founded and well protected from direct effects of solar radiation. When the instruments were side by side they gave similar and practically identical results.

The level, which was in charge of Mr. H. G. Morgan Hobbs, resident at Sunnyside, was read three times a day from July 26 to August 27, 1905. The difference between successive readings usually varied between 0″·5 and 3″·0, and with but few exceptions these readings indicated changes in level in opposite directions. During wet weather the diurnal movement was eclipsed by a rapid movement in a direction which corresponded with a closing of the valley. This sometimes amounted to 18″·0.

In the Geography Section a discussion was opened by Major E. H. Hills, C.M.G., R.E., on "Proposed Remeasurement of Geodetic Arcs in the United Kingdom."

The fundamental triangulation of Great Britain and Ireland was completed fifty years ago. Though excellent work for the time at which it was done, it is now far behind the standard of modern work of its class.

The result of this inferiority is that the British work cannot be co-ordinated with the Continental series for the purpose of geodetic discussion, such as a determination of the figure of the Earth. This defect is all the more noticeable in that the necessary observations to connect the two series were actually made at considerable expense. To remedy this defect, which may justly be described as a standing reproach to British science, it is by no means necessary or desirable to reobserve the whole original network of triangulation. What is required is to connect geodetically, by as good a set of triangles as can be selected, the three extreme points of our islands, viz.—(1) Saxavord, the northernmost point of the Shetlands; (2) Valentia, on the west of Ireland; (3) the stations on the coast of Kent trigonometrically connected with France.

Were this work done we should have completed, as far as lies in our power, two very important lines of geodetic triangulation—namely, the meridional arc, through the longitude of Greenwich, and the longitudinal arc, along the parallel of 52° north latitude.

At present, without including Great Britain, these arcs extend from the north-east corner of France to Ain Sefra in Algeria and to Orsk in Russia respectively, amplitudes of 18° and 57°.

With the British work there would be available added amplitudes of 10° and 11½° respectively. Geodetic work is now much more rapid, as well as more accurate, than when the triangulation of England was done. The invention of invar has enormously simplified the difficult operation of base measurement, and the theodolites, for making the angular measurements, can now be obtained more precisely graduated and of a design altogether superior to the older forms. The general result of these improvements is that, while a much higher standard of accuracy can be realized, the rate of progress of such work has been increased in an even higher ratio.

---

* See Brit. Assoc. Rep. 1895, p. 132.

The work should be done by the Ordnance Survey Department, which is, it is believed, quite ready to undertake it; and the powerful influence of the British Association might justifiably be exerted in persuading the Treasury to grant the requisite funds. The total cost would be quite trifling compared with the existing national expenditure upon survey work.

In the Department of Section A devoted to Astronomy and Cosmical Physics, Prof. TURNER read a paper on "Rainfall Periodicity."

No law has been discovered of forces that control the rainfall. Prof. Schuster suggested a year or two ago that the figures for various stations should be subjected to careful analysis to see whether there was any periodicity and whether there were sets of years, months, or days in which something like regular variation could be traced. Prof. H. H. Turner stated the results so far of an effort which he and Prof. Schuster had made to discover this periodic law, if there be one. They took the rainfall of Padua, of which there is extant a record of 175 years, and that of Greenwich Observatory, for which data exist for about sixty years, and subjected them to the "periodogram" analysis. The labour was enormous, and the outcome for a long time unsatisfactory. They took periods of twenty-one months down to six months with purely negative result, that is to say, without finding any trace of periodicity, but, curiously enough, when they came to five months there did seem to be regular, or nearly regular, progression in the rainfall. There was, for example, generally an excess in the beginning, a deficiency in the middle of the period, and then a return to excess—that is to say, not a constant but an average return of maxima and minima. The investigators also traced a shorter period of twenty-five days with an average periodicity of the same kind. It may interest meteorologists who keep records of rainfall to test these periods by those records. The results were not quite uniform. They held good for the twenty years from 1905 backwards to 1885, though not in the next twenty from 1885 to 1865, but they did hold in the next series of years. The records examined with these results were those of Greenwich. The inquiry is but just begun and no great weight can yet be attached to it, but, as Prof. Turner observed, the issue would be something of value if it merely showed that no periodicity was actually traceable.

Prof. Turner also made some remarks on ground vibrations caused by railways in the neighbourhood of an Observatory, especially referring to the Armagh Observatory (see August number, p. 336).

Father Cortie gave an address in the same Section on the connection between the disturbances of the solar surface and the corona, and

The **Earl of Rosse** gave an account of observations which he has made to test the amount of radiation from the Moon's surface.

On the San Francisco Earthquake of 1906, *April* 18.

[Number 108 of the 'Publications of the Astronomical Society of the Pacific' may be called the earthquake number. First we have a ten-page article by F. Omori, entitled "Observations of Distant Earthquakes," which is really a complete and semi-popular exposition of seismic records. Then follows a picture of the Ewing Seismograph Record of the Lick Observatory, 1906 April 18, with an explanation by J. D. Maddrill. The Director of the Observatory then gives an account of the earthquake and its effects at Mount Hamilton. This we print in full.]

It is not necessary to say that the earthquake of April 18th was very severe on Mount Hamilton, but it is a matter for congratulation that the Observatory suffered no serious injury. Director Campbell was in Washington, D.C., at the time, and Astronomer Tucker was in charge. It was found that the telephone connection with San José was destroyed, and, as fires were observed to break out at once in San José and San Francisco, the seriousness of the situation was soon realized. Prof. Tucker dispatched a bicycle messenger to San José, bearing the message that "the buildings and instruments of the Lick Observatory were not injured by the earthquake." All was confusion in San José, and there was no telegraph or telephone communication with the outside world. The messenger therefore rode on to Berkeley, 75 miles from Mount Hamilton, and delivered a copy of the message to President Wheeler, of the University, late in the afternoon of the 18th. As it was not possible to send private dispatches, the message was delivered to the Associated Press, with the request that it be sent over the wires as soon as possible. This was done on the 19th, and the message was correctly published in many eastern papers of the 20th. A Chicago paper containing it overtook me in Iowa on the forenoon of the 20th, as I was speeding westward, and the relief afforded will not soon be forgotten. It was hoped that this information would reach all inquiring friends of the Observatory, both in this country and abroad; but there soon appeared to be considerable doubt of this. On this account the message was, at our request, repeated a week later by the Associated Press.

The details of the earthquake are given for Mount Hamilton in Mr. Maddrill's note. The maximum double amplitudes of the motion here could scarcely be less than, and perhaps exceeded, three inches. Fortunately, the period of vibration was long, and the buildings and instruments apparently had time to follow the earth-motions. The tops of three large and one small brick chimneys were cracked, but not broken off. A great many old cracks in plastering were reopened, and many new cracks were formed. These apparently comprise the damage on the mountain. The brick and cement reservoirs and the water systems were not affected. Prof. Tucker's observations with the meridian-circle established that the instrument's position had not shifted appreciably. At my request, Dr. Aitken and Dr. Moore determined the position of the polar axis of the 36-inch refractor. They found no significant change.

This immunity, so to speak, is somewhat surprising in view of the fact that the damage in San José, only thirteen miles directly west of us, was severe, and that in regions thirty to fifty miles east of us a large proportion of chimney-tops were thrown down. Altitude alone can scarcely be a protective factor. The breaks in the rock-strata between this Observatory and the origin of disturbance —there are two narrow valleys between us and San José—are probably potent factors in reducing intensities. It is a familiar fact that earth-waves of great destructiveness on one side of a mountain-range often reappear destructively on the other side, as in the present case, with only minor destruction in the mountains themselves; but the explanation of these phenomena is usually not apparent. There can be no doubt, however, that the rigid rock-foundations of our buildings and instruments were exceedingly important elements in their stability under the severe strains suddenly thrown upon them.

The indirect effects of the catastrophe upon the finances of the Lick Observatory and other State institutions cannot now be determined. Even though a way be found to maintain present incomes, there can be no doubt that further development, from State funds, will be seriously limited for many years to come. This is in no sense a statement of discouragement, as it is hoped that, as in the past, other sources of income may present themselves as needs arise.

The earthquake was destructive on a narrow belt extending along the coast from near Eureka in the north to Salinas in the south, a distance of about 300 miles. More than half the people of the State and more than half the property are located in the affected district. The direct earthquake destruction was a minor matter, perhaps not exceeding $35,000,000, but the fire in San Francisco destroyed property valued at between $500,000,000 and $800,000,000, or, roughly, one-fourth or one-fifth the assessable property of the State. One-third to one-fourth of this loss will probably be borne by insurance companies in other States and countries. Those State institutions which depend upon *pro rata* taxation for their support have lost a corresponding proportion of their income. The University of California loses $50,000 or $60,000 in this manner. It lost, besides, uninsured business property in San Francisco, whose annual rental was $60,000. The State of California will, no doubt, repair these losses, as far as it is financially able; and there are no people in the world more ready to support higher education in all its forms and phases, to the extent of their ability, than are the people who compose the commonwealth of California. They are both unusually practical and unusually interested in the ideal.

Four-fifths of the normal income of the Lick Observatory consist of State funds, assigned to it by the Regents of the University. These financial statements are made in deference to many inquiries from distant friends.

On the day following the earthquake, His Excellency Governor Geo. C. Pardee appointed a State Commission to investigate the scientific aspects of the phenomenon. The Commission consists of Prof. A. C. Lawson, University of California, Chairman; Prof. J. C. Branner, Stanford University; Prof. George Davidson, University of California; Prof. Harry Fielding Reid, Johns Hopkins University; Dr. C. K. Gilbert, U.S. Geological Survey, Washington; Prof. A. O. Leuschner, University of California, Secretary; and Director W. W. Campbell, Lick Observatory, University of California.

The Carnegie Institution of Washington has generously undertaken to defray the expenses of the investigation.

The work of the Commission is well organized and many facts of extreme interest have been established. The principal fact is that the earthquake was due to very extensive motion of the strata along a well-known geological fault that runs near the coast-line. The motion was principally of the horizontal-shearing type, with few apparent evidences of any vertical component. The geological members of the Commission have traced the *surface fissure* along the fault-line practically continuously from the mouth of Alder Creek, just north of Point Arena, south to Fort Ross, where the fissure enters the ocean; thence from its reappearance in Tomales Bay, south to Bolinas Bay, where it again enters the ocean. It undoubtedly passes under the ocean about two miles out from the Golden Gate, as it re-enters the land some six or eight miles south of the Golden Gate and follows the old fault-line continuously south-south-east to a point a short distance south-east of Salinas. On the east side of the fissure the strata have moved south, and on the west side they have moved north, relatively. The relative motion appears to be a maximum in the Tomales-Bolinas region, about 20–25 miles north-west of San Francisco. A public road crossing the fault-line at right angles has now an offset of seventeen feet at the fault-line. A large tree standing exactly on the east edge of the fissure is now twenty-four feet south from the small roots which it left in the west bank of the fissure. A path crossing the fissure is offset eighteen feet; three trees formerly stood in a row about seven feet east of a small house. The fissure passes between them and the house, and they are now eighteen feet southerly from the house. A barn is situated exactly over the fissure, some four-fifths of it being on the west side. Although badly wrecked, the superstructure remains with its foundation west of the fissure, but that part of the foundation lying east of the fissure has moved southward under the barn through eighteen feet. A fence crossing the fault-line is offset eighteen feet at the fissure. About twenty miles westward from Mount Hamilton, on the fault-line, the offsets are about eight and one-half feet, as a maximum. All the offsets referred to are in the thick loamy soil, and it is possible that, on account of lag in the soft soil, the shear in the underlying rock stratum is larger.

Many interesting questions are raised as to disturbances in boundary-lines between farms, in latitudes, azimuths, &c.

The same geological fault extends south-easterly throughout the State to the vicinity of the Colorado River, and perhaps further.

W. W. Campbell.

The Sun-spots of 1906.—II.

The general character of the sun-spot activity of the first semestre of 1906 has been that of the last phase of maximum. The spot-groups have been numerous, especially in the northern hemisphere, but the tendency has been rather to develop long processions of groups of moderate size following each other at short intervals along a parallel of latitude than to concentrate into great compact disturbances. The activity has, on the whole, tended to increase with the progress of the year, but as its chief manifestations have been confined to the neighbourhood of a few meridians, it has advanced by a series of irregular oscillations.

The following table gives the current numbers and dates of commencement, reckoned in Greenwich civil time, of the solar rotations for the year 1906:—

No. of Rotation.	Date of commencement.	No. of Rotation.	Date of commencement.
	d		d
699	1905 Dec. 26·08	706	1906 July 5·02
700	1906 Jan. 22·42	707	Aug. 1·23
701	Feb. 18·77	708	28·47
702	Mar. 18·09	709	Sept. 24·73
703	April 14·38	710	Oct. 22·02
704	May 11·62	711	Nov. 18·33
705	June 7·83	712	Dec. 15·64

Spot-Groups in 1906, April to June.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	d	°	°	
5837	Mar. 27	Apr. 7	12	Apr. 1·5	170	−15	Fine stream.
8	29	Mar. 29	1	2·9	152	− 7	Very small spot.
9	30	Apr. 4	6	Mar. 29·4	211	+10	Irregular stream.
5840	Apr. 1	1	1	29·9	204	+13	Few very small spots.
1	1	4	4	30·2	200	+ 7	Straggling stream.
2	1	6	6	Apr. 1·9	165	+12	Straight stream.
3	1	11	11	6·3	107	+20	**Return.** Regular.
4	2	2	1	Mar. 27·7	234	+20	Two small spots.
5	2	2	1	Apr. 1·4	171	−20	Two small spots.
6	2	4	2	3·3	146	− 7	Small. Intermittent.
7	4	10	7	8·5	77	+19	Small stream.
8	5	5	1	9·1	69	+19	Small faint spot.
9	5	16	12	11·1	44	+23	**Return.** Irregular.
5850	7	8	2	8·6	76	+ 8	Small sparse stream.
1	8	8	1	4·0	137	−16	Very small spot.
2	8	18	11	12·8	21	−19	Two spots.
3	9	12	4	12·6	24	+ 8	Regular spot.
4	12	19	8	14·1	4	+ 9	Two spots.
5	15	26	12	20·8	275	+16	**Return.** Large cluster.
6	16	17	2	12·5	25	+15	Two small spots.
7	17	20	4	18·4	307	−22	Few very small spots.
8	24	29	6	29·5	160	+14	Few small spots.
9	25	May 4	10	28·5	173	+12	**Return.** Irregular.
5860	26	Apr. 29	4	May 1·3	136	+23	Few very small spots.
1	26	26	1	Apr. 21·6	265	−33	Two very small spots.
2	27	27	1	22·2	256	+16	Two spots.
3	27	May 3	7	30·1	153	+12	Short stream.
4	May 1	5	5	30·7	145	+23	Small irregular stream.
5	4	15	12	May 10·5	15	+ 6	**Return.** Large, regular.
6	5	17	13	11·3	5	−23	Large, regular, with train.
7	6	17	12	12·0	355	+13	Few very small spots.
8	7	10	4	10·9	9	+11	Few very small spots.
9	13	22	10	15·8	305	+12	Very fine compact stream.
5870	19	30	12	25·1	182	+ 9	**Return.** Irreg. stream.
1	23	June 4	13	28·9	132	+11	Two large spots.
2	24	2	10	27·1	155	+ 7	Small stream.
3	24	May 30	7	30·4	112	+17	Few small unstable spots.
4	25	27	3	27·9	145	+26	Few small unstable spots.
5	27	27	1	22·3	218	+13	Small cluster.
6	28	June 2	6	30·9	105	−12	Small stream.
7	June 1	4	4	June 4·0	51	−13	Few small unstable spots.
8	2	10	9	5·5	31	+16	Stream.
9	2	2	1	1·9	78	−17	Small spot.
5880	2	2	1	7·9	359	+15	**Return.** Small, faint.

Spot-Groups (*continued*).

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	d	°	°	
5881	June 2	June 10	9	June 7·9	359	−25	**Return.** Small, unstable.
2	3	10	8	7·2	8	−17	Some small unstable spots.
3	3	10	8	8·2	355	−22	Some small unstable spots.
4	4	7	4	10·1	330	+19	Small, regular.
5	5	18	14	12·0	305	+15	**Return.** Large, regular.
6	8	14	7	7·5	5	+20	Large stream.
7	7	7	1	15·2	262	− 9	Cluster.
8	19	23	5	22·0	173	+ 9	Irregular cluster.
9	19	30	12	24·8	135	+11	**Return.** Small, regular.
5890	21	21	1	16·6	244	−29	Small spot.
1	21	21	1	22·1	171	−20	Small spot.
2	22	30	9	26·6	112	+ 4	Two large spots.
3	24	July 5	12	30·1	65	+ 6	Regular, with train.
4	26	7	12	July 1·7	44	+12	Cluster.
5	26	7	12	1·9	41	−11	Large double spot and train.
6	28	4	7	June 29·2	77	+22	Short stream.
7	28	4	7	July 2·6	32	+17	**Return.** Small, regular.
8	28	10	13	4·5	7	+21	**Return.** Large, regular.
9	29	June 29	1	2·1	39	+15	Few very small spots.
5900	29	29	1	2·2	38	+ 9	Few very small spots.
1	29	July 8	10	4·3	9	+10	Faint irregular cluster.
2	30	2	3	June 29·9	68	+12	Fine stream.

Notes on the Principal Spot-Groups.

Group 5837.—A fine stream consisting of a large regular spot, followed by a long train of small spots. Area of the group about 400, expressed, as usual, in millionths of the Sun's visible hemisphere.

Group 5843.—Return of Group 5817. The group had passed the stream stage, and was now a stable regular spot; area about 120.

Group 5849.—Return of Group 5820. The group was still in the active stage, and did not become a stream of the usual type. The largest member of the group was in the rear, a large regular spot, and the group as a whole tended to form a zigzag across a parallel of latitude, rather than extend along it. It varied in area, being about 450 at its greatest development. It diminished about April 13, but revived again later.

Group 5855.—Return of Group 5826. In the diminishing phase, though with some oscillations in size. Area on April 20 about 360. A large cluster at first, of which a close pair of large regular spots formed the chief member. This pair alone remained after April 20.

Group 5859.—Return of Group 5842; Group 5858 being probably also part of the same disturbance. Much increased from the previous apparition. An irregular stream undergoing great and frequent changes. Area on April 26 about 180.

Group 5865.—Return of Group 5854. Much increased from the previous apparition, but now in the diminishing phase. A large stable regular spot, area on May 8 about 200.

Group 5866.—A large stable regular spot, closely followed by a train of small spots, making a neat stream. Area about 450.

Group 5869.—A very interesting group, commencing as a nearly circular ring of spots, enclosing a great area of the surface. The spots of the ring drew out to form a very fine compact stream, of which the first and last spots were the largest. The active phase. Area on May 15 about 600.

Group 5870.—Third apparition. Return of Group 5859. Much increased from the two previous apparitions. A long irregular stream, divided into three principal sections. The leader spot was large and well-defined, and a number of faint spots formed immediately to the south of it. The last section consisted of a few small spots widely scattered. The area of the group was about 440 on May 27, after which it diminished.

Group 5871.—Two large spots with a few small companions. The leader was a double spot at first, but the two portions completely coalesced. The rear spot gradually broke up. The area of the group gradually diminished from about 450 on May 27 to 300 on June 2, and 100 on June 4.

Group 5872.—A small stream slowly increasing.

Group 5881.—Return of Group 5866 in a greatly diminished form, as a number of small scattered unstable spots. Groups 5882 and 5883 lay a little to the north of Group 5881, and were of a similar character. The three groups together made up a wide area of disturbance.

Group 5885.—Return of Group 5869 in a diminished form. It was now a large regular and stable spot, occasionally with a few small spots following it in a straight stream. Area about 300.

Group 5886.—A fine stream, area about 500, which formed suddenly.

Group 5889.—Return of Group 5871 in a greatly diminished form, as a small regular spot, of area about 30.

Group 5892.—An increasing group. At first a small regular spot followed by a still smaller companion, it developed into a stream of the normal type; two large spots with a few small spots connecting them.

Group 5893.—A diminishing group. A large regular spot followed by a long train of small spots. Area:—June 27, about 150; July 2, about 90.

Group 5894.—A cluster of small spots. Diminishing.

Group 5895.—A large double spot followed by a train of small spots. An increasing group. Area about 300 on July 2.

Group 5897.—Return of Group 5878 in a diminished form, as a small regular spot, area about 40.

Group 5898.—Return of Group 5886 in a greatly enlarged form, as a very large regular spot; area about 800.

Group 5901.—A number of faint spots in an irregular cluster, area about 150.

Group 5902.—A remarkable outburst; a fine stream, of area about 100, appeared very suddenly between Groups 5893 and 5896. E. WALTER MAUNDER.

The new Minor Planet TG.

ATTENTION has already been called in these columns to the remarkable orbit found for this planet by Dr. Berberich, which indicated a mean distance and period almost identical with those of Jupiter, thus suggesting that the perturbations by the latter would be extremely interesting. Prof. C. V. L. Charlier has written a very interesting article in *Astr. Nach.* 4094, in which he suggests that the Sun, Jupiter, and TG may form a system of 3 bodies in permanent stable equilibrium. Lagrange showed that if 3 masses (equal or unequal) were situated at the angular points of an equilateral triangle, and projected with a certain angular velocity, the same for all, about their common centre of gravity, they will continue to revolve with uniform velocity, always forming an equilateral triangle of unvarying size, so long as they are subjected to no forces other than their mutual actions. This interesting result has not hitherto received the attention that it deserves, since it was looked upon as a mere theoretical result, with little prospect of its practical application. As the proposition is omitted in many of the ordinary text-books of dynamics, and is capable of being established in a few lines, it may be worth while to give the proof. Let S, J, T be three bodies whose masses are s, j, t, situated so as to form an equilateral triangle of side a; then the accelerations produced on T by S and J are

$$\frac{ks}{a^2} \text{ along TS}, \qquad \frac{kj}{a^2} \text{ along TJ},$$

or

$$s\,.\,\mathrm{TS}\,.\,\frac{k}{a^3} \text{ along TS}, \qquad j\,.\,\mathrm{TJ}\,\frac{k}{a^3} \text{ along TJ}.$$

Let M be the centre of gravity of S, J, and G the centre of gravity of S, J, T; then by a well-known proposition the above accelerations compound into

$$(s+j) \, . \, \mathrm{TM} \, . \, \frac{k}{a^3} \text{ along TM.}$$

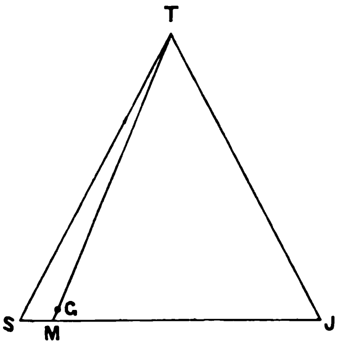
Which is equivalent to

$$(s+j+t) \, \mathrm{TG} \, . \, \frac{k}{a^3} \text{ along TG.}$$

By symmetry the accelerations of S, J are

$$(s+j+t) \, \mathrm{SG} \, . \, \frac{k}{a^3}, \qquad (s+j+t) \, \mathrm{JG} \, . \, \frac{k}{a^3}$$

along SG, JG respectively.



Now if the three bodies were rotating about G with uniform angular velocity ω, their velocities would be ωGT, ωGS, ωGJ, and their accelerations ω^2GT, ω^2GS, ω^2GJ, along TG, SG, JG.

Hence if ω^2 be equal to $(s+j+t) \, . \, \frac{k}{a^3}$, the three bodies will continue to rotate uniformly about G under their mutual attractions.

It will be seen that the result holds whatever be the law of Force, provided it is merely a function of the distance, not of the direction.

The two facts which lead Prof. Charlier to suggest that such may be the character of the motion of Jupiter and TG are: (1) the period found for TG by Dr. Berberich is 12·02 years, only 0·16 years in excess of Jupiter's period; it is quite possible that the period is in error by this amount and is really exactly the same as that of Jupiter: (2) the heliocentric angle between the planets approximates to 60°, being 55° 31′ on 1906 February 22.

It is, of course, obvious that the system of bodies cannot move in quite such a simple manner as is contemplated in the above proposition; for the orbits of Jupiter and TG are both decidedly elliptical, and their planes are not coincident, but inclined to each other at an angle of 10° or thereabouts. Prof. Charlier, however, has shown that such causes would merely cause oscillations about the equilateral configuration, and would not lead to its permanent derangement. He has investigated in considerable detail the case where the plane of TG's orbit is considered as identical with that of Jupiter, and finds that the motion of TG under these conditions may be thus summarized: Consider a point moving round the Sun in an ellipse in accordance with Kepler's Laws in a period equal to that of Jupiter (11·86 years), but with a direct motion of the perihelion so that the anomalistic period is 11·90 years.

Then TG moves round this point in a small ellipse in a period of 148 years, the dimensions of this small ellipse not being determinable till TG has been longer under observation. When the mutual inclination of orbits is taken into account there will in addition be a latitude term with a period differing slightly from 11·86 years, and I suppose a smaller term of much longer period. The action of Saturn on the system will be of interest; there will, of course, be a "long inequality," whose period will be identical with that produced on Jupiter, but whose phase and amplitude will be different.

The planet TG is now invisible, but will be reobservable at the end of the year, and a few observations made then should be sufficient to decide whether the period is identical with that of Jupiter. It is greatly to be hoped that such may be the case, as it would add to the solar system an interesting "new case in celestial mechanics," foreseen by theory, but hitherto unknown in fact. Prof. Charlier suggests that diligent search should be made in the two regions of the ecliptic distant 60° from Jupiter, as it is quite possible that he may have more than one of these tiny appendages. He extends the suggestion to Saturn, but the prospects of successful search in his vicinity do not appear to be very bright.

It will be remembered that some 2½ years ago Prof. Stockwell published in *Ast. Journ.* 557 a paper on the mutual perturbations of two planets moving in the same circular orbit round the Sun, in which he drew the conclusion that whatever the initial distance might be, they would adjust themselves so as to form with the Sun an equilateral triangle, and remain there in stable equilibrium. Dr. E. Strömgren criticised this result in *Ast. Nach.* 4015, stating that Prof. Stockwell's investigation only included the first order of the masses, and treated a certain angle as constant which should have been treated as variable. From his own researches combined with those of Dr. Charlier he drew the conclusion that the equilateral configuration was stable within certain limits, but that beyond these limits the bodies would recede further from the equilateral form instead of approaching it. Dr. Strömgren pointed out one case of especial interest where the equilateral configuration would be continually approached in an asymptotic manner; that, however, required a certain condition to be exactly fulfilled, which would be scarcely likely to be realized in practice.

A. C. D. CROMMELIN.

The Planets and Planetary Observation.

[Continued from p. 314.]

MERCURY.

THIS planet is chiefly interesting to observers as an object for the naked eye, rather than as an orb presenting surface-markings suitable for telescopic study. The amateur usually looks for Mercury for the mere pleasure of detecting him and watching his strongly glittering lustre, exhibiting, as it usually does, a decidedly rosy tint in the evening twilight. When found the simple contemplation of the object affords sufficient satisfaction without bringing the telescope into use at all. For even when good instruments are turned upon Mercury one generally sees only a small moulding disc, ill-defined and momentarily disturbed by the vapours running across it. The approximately half-moon shape which it presents near greatest elongation can scarcely be recognized amid the contortions it is ceaselessly undergoing. Of course this is the worst side of the picture and alludes to the planet's appearance when he is visible to the naked eye and near the horizon. Certainly there are times when the little disc appears fairly well defined, when the phase can be very distinctly traced, and when some irregular dusky markings become clearly outlined. Results of this kind are usually obtained in daylight a short time after sunrise or just before sunset. At such times the planet is often sufficiently high above the dense disturbing vapours floating about at low altitudes to display a steady image, and it is only under such conditions that the telescopic examination of his surface can be successfully effected.

Some of our astronomical handbooks refer to the detection of Mercury as a very difficult feat and relate how Copernicus lamented, on his death-bed, that he had never caught a glimpse of the planet.* But there is no doubt that Mercury can be distinguished more easily and more frequently than many writers affirm, and that the story of Copernicus's failure is little more than a legend; it at any rate forms a doubtful incident. Copernicus resided at Thorn in Prussia, and it has been said that on the borders of the River Vistua, running near, fogs frequently obscure objects at low altitudes and might prevent Mercury, near his rising or setting, from being seen. But whatever may have been the experience of Copernicus, it is certain that the statement in question has imparted an interest to the planet which it would otherwise have lacked. Hundreds of expectant eyes scan the western horizon after sunset in the spring months whenever Mercury is near his easterly elongation, and should the fugitive little planet be picked up the spectacle never fails to afford genuine gratification.

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* This statement is made in Humboldt's 'Cosmos' and reiterated in the works of Arago, Smyth, Chambers, and others. An interesting letter on the subject was written by Mr. Lynn in the *Observatory*, xv. p. 321, and the details he gives practically negative the usually accepted version. In discussing the motions of Mercury, Copernicus utilized observations made at places having the advantage of clearer skies than those prevailing in his own neighbourhood, but that he ever complained of absolute failure to see the planet for himself rests on very slender testimony.

The late Rev. S. J. Johnson made it a point to look out for Mercury at favourable times, and viewed him as an evening star with the naked eye about 150 times during the years 1858-1905. The writer at Bristol has seen the planet 130 times since Feb. 1868, but has not availed himself of many opportunities. Probably this object could be seen on about 15 occasions annually in the English climate, by anyone who made it a regular practice to search for him. In the spring apparitions Mercury should be looked for *before* the date of maximum elongation, and in autumn *after* the elongation. The alteration of phase is rapid, and though near E. elongation the planet is approaching the Earth the decrease in the illuminated portion of the disc is so great that the increased diameter does not nearly compensate, and a considerable decline in lustre becomes apparent. The planet is sometimes quite a striking object 10 evenings or so before max. E. elong., but may be utterly invisible a week or so after passing that position.

To show the differences in the extent of surface illuminated, in the apparent diameter, and in the distance of the planet from the Earth, the following values are given from data in the *Nautical Almanac*, 1907:—

Max. E. Elongation, 1907 March 1^{d} 14^{h}.				Max. W. Elongation, 1907 August 12^{d} 15^{h}.			
Date.	Portion of disc illuminated.	Apparent diameter.	Distance from Earth in millions of miles.	Date.	Portion of disc illuminated.	Apparent diameter.	Distance from Earth in millions of miles.
1907.		″		1907.		″	
Feb. 24	·712	6·2	100	Aug. 8	·249	8·2	73
25	·672	6·4	98	9	·279	8·0	75
26	·629	6·6	95	10	·311	7·8	77
27	·586	6·8	93	11	·345	7·6	80
28	·542	7·0	90	12	·382	7·4	82
Mar. 1	·499	7·2	87	13	·421	7 2	84
2	·455	7·4	85	14	·461	7·0	86
3	·412	7 6	82	15	·502	6·8	89
4	·368	7 8	80	16	·643	6·6	91
5	·325	8·0	77	17	·583	6·4	94
6	·282	8·2	75	18	·622	6·3	97

Thus though at the E. elongation the illuminated area of the disc decreases from ·712 to ·282 in 10 days, the diameter increases only from 6″·2 to 8″·2. At the W. elongation though the illuminated area increases from ·249 to ·622 in 10 days, the apparent diameter decreases only from 8″·2 to 6″·3. But it is not merely the difference in phase but the difference in the intensity of sunlight transmitted from Mercury which induces the variable brightness of the planet, for with the disc rather small and about three parts illuminated the nearly vertical Sun would give much more power than in the slanting beams at the time of the crescented phase. These facts show why Mercury appears brighter preceding his E. elongations and following his W. elongations. He would be a fine object were the reflecting-power of his (probably very thin) atmosphere greater and equal to that of some of the other planets. Proctor, in his 'Other Worlds,' shows that if Mercury had the same capacity of reflection as Jupiter the former ought to appear, when in perihelion and at his greatest elongation from the Sun, twice as brilliant to the naked eye as Jupiter does under the best conditions. The albedo of Mercury is only 17, that of Venus and Jupiter about 65, while that of clouds is about 72.

The naked-eye aspect of Mercury varies a good deal according to several circumstances. Apart from the phase, the state of the atmosphere and the intensity of the twilight in which he is involved affect his apparent lustre very materially. In a May or June sky he is rarely seen so well as in Feb. or March. From many experiments the writer has found that under normal conditions he becomes visible to a good eye about 30 min. after sunset, and is brightest, when the western quarter is clear right down to the horizon, about the same period before his setting. At times he has shone nearly as brightly as Jupiter, but with a flashing lustre, in singular contrast with the steadiness of the Jovian orb. On other occasions (when in a crescented phase) he has been quite invisible even in a beautifully transparent sky and on a night when he remained above the horizon nearly two hours after sunset.

The disadvantages attending telescopic observation of Mercury are such that comparatively little is certainly known as to his surface-markings. Dark and light features have been detected, but somewhat dubiously, and the actual topography of his disc still awaits a reliable observer and delineator. Definite markings appear to have been first seen on Mercury by Schröter, of Lilienthal. He observed with his 13-in. reflector a blunting of the S. horn, an irregular contour of the terminator, and projections from the limb. From these appearances he inferred the planet's surface to be mountainous, and one range Schröter measured as 58,000 feet (11 miles) in elevation. The rotation of the planet in little more than a terrestrial day was derived from these results.

That the conclusion merited confidence was proved by observations of a dusky streak and blotch in 1801 May and June, which exhibited a nearly similar rate of motion. Schröter observed the planet diligently between 1780 and 1815, and the results were detailed in his 'Hermographische Fragmente.' He describes the terminator of Mercury as much dimmer than the outer circular portion. He also found the illuminated portion of the disc less than it should be theoretically, and Mädler, in later years, corroborated this fact.

Prince observed a bright spot on Mercury on a very favourable night—1867 June 11. The marking was just S. of the centre and had faint lines diverging from it N., E., and S. Birmingham suspected the existence of a large white spot on the E. limb in 1870 March 13, and Vogel observed markings in 1871 April 14 and 21.

In 1882 Nov. 5, 6, 8, and 9, I observed Mercury at about the time of sunrise, employing a 10-in. reflector, power 212. Dark patches and bright spots and veins were detected on Nov. 5, 18^h 20^m–50^m. The planet showed a well-marked luminous area between the E.N.E. part of the limb and terminator. Three dusky condensations were seen near the terminator, and the S. horn appeared distinctly blunted; the S. half of the crescent was generally darker than the N. On November 6, 18^h 25^m–55^m, the appearance was very similar. The darkest portion of the planet was near the S. horn. On November 8, 18^h 35^m–19^h 30^m, definition was excellent and the markings readily observed. The bright area seen on previous nights was still visible, and placed centrally within it appeared a small brilliant point with luminous radiations. On the S.W. border of the bright area there was a dark marking, and another, even deeper in tone, inlying the blunted S. horn. The aspect of the markings reminded me forcibly of Mars. On November 9, 18^h 40^m–19^h 39^m, definition was again very good, and most of the features were re-detected, though they were placed obviously to the eastward of their positions on previous mornings, proving that the rotation-period of the planet is somewhat greater than 24 hours. The dark patch near the S. horn, noted on November 8, now appeared, in its chief intensity, near the limb, and the bright area was decidedly fainter and situated not far within the E. margin. There was a conspicuous dark marking nearly central over the terminator, and other faint ones were noticed towards the horns. At times I fancied there was a small bright spot at the extremity of the N. horn, wholly detached from the rest of the disc, but the appearance was attributed to disturbances of the image, the rippling at the cusp being quite sufficient to cause such an illusion.

I sent Sig. Schiaparelli, of Milan, a summary of my observations, and he replied saying that he agreed as to the general similarity between the markings of Mercury and Mars. He described the former as having "spots which become partially obscured and sometimes completely so; the disc also displays brilliant white spots in a variable position." He examined the planet entirely by day when near the meridian and saw markings on many occasions, but not always with the necessary distinctness to enable accurate drawings to be made. Some of the observations were prolonged over 8 successive hours. He found that the views he obtained near superior conjunction were more instructive than those at elongation when the disc was near dichotomy, the defect in diameter being compensated by the whole disc being visible and more strongly illuminated than under other conditions. He used an 8½-inch refractor in 1882, and his later researches were supplemented by an 18-inch. The number of his observations may be judged from the fact that 150 drawings were made in the years 1882 and 1883. The larger instrument was found to have no advantage over the smaller one.

In *Ast. Nach.* 2944 Sig. Schiaparelli gave the results of his investigations and stated that from a discussion of all the data he concluded that Mercury rotates on his axis in 87·969 days, just as the Moon revolves round the Earth, presenting to the Sun always the same hemisphere. One of his best drawings was obtained when Mercury was only 3° 2′ from the Sun's limb, the diameter of the planet being between 4″ and 5″. He thought it highly probable that Mercury had a very dense and imperfectly transparent atmosphere, and suggested that the unequal brightness of the southern cusp, which is usually much less luminous than the northern, must have given rise to Schröter's supposed observations of lofty mountains. The spots, which appeared as very faint, darkish streaks of a reddish-brown colour, seemed always to be the same in position and figure. They became effaced on the limb as though through atmospheric absorption. The disc of the planet was, during the best definition, of a clear rose-colour verging towards copper; this rose tinge had been previously remarked by Harding and De la Rue.

In 1896 Herr Brenner, of Lussiupiccolo, Istrien, observed Mercury * every day after his inferior conjunction and made 16 drawings showing spots on the planet. These spots convinced him that Mercury rotates much quicker than Sig. Schiaparelli believes, although a little slower than the Earth and Venus. He saw polar spots similar to those on Mars, and the variations were incompatible with a long rotation-period of 88 days. From an exhaustive investigation of the spot-motions he concluded that a period of 33 to 35 hours ($33^{h}{\cdot}75$) represented them fairly well. He recognized certain individual markings, seen by him in Schiaparelli's chart of Mercury (*Ast. Nach.* 2944), but says that Mr. Lowell's canals on Mercury (*Monthly Notices*, lvii. 148) were not seen by him and probably had their origin in some optical deception similarly to the supposed canals on Venus. Herr Brenner employed a refractor of 7 inches aperture, powers 196 to 410.

[To be continued.]

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* 20 drawings are given in *Ast. Nach.* 3387, and *B.A.A. Journal*, vii. 391 (1897 June).

The Spectra and Colours of Stars.

In No. 3980 of the *Astronomische Nachrichten* Dr. Möller, of Elsfleth, gives his observations of the colours of 169 bright stars in the northern and southern hemispheres. I thought that a comparison between these results and the spectra as found at Harvard would be interesting. Dr. Möller says that his observations were made on board a sailing-ship in the tropical parts of the Atlantic Ocean, and in calm seas, between October 1903 and March 1904. As he says nothing about the spectra of the stars he observed, I presume that his estimates of the colour were made independently of any knowledge of the spectra. A comparison between the colours and spectra—both determined independently—should therefore prove of interest. The estimates of colour were made with the naked eye and an opera-glass. The faintest star observed was about 4½ on the Harvard scale of photometric magnitudes.

The scale of colour used by Dr. Möller was as follows:—

1. Pure white (rein weiss).
2. Yellowish white, white preponderating (gelblich weiss, weiss überwiegt).
3. Yellow and white in equal proportions (zu gleichen Teilen).
4. Whitish yellow, yellow much preponderating (gelb überwiegt sehr).
5. Pure yellow (rein gelb).
6. Reddish yellow, yellow much preponderating.
7. Red and yellow in equal proportions (zu gleichen Teilen).
8. Yellowish red, red much preponderating.
9. Pure red (rein rot).

The following is a summary of the results of a comparison between the colours and spectra:—

No. of Stars.	Spectrum, Harvard.	Mean colour, Möller.	No. of Stars.	Spectrum, Harvard.	Mean colour, Möller.
2	Oe 5 B	2·25	7	F	4·9
7	B	2·7	1	F 2 G	5·0
9	B 1 A	2·74	5	F 5 G	4·6
10	B 2 A	3·0	3	F 8 G	5·8
13	B 3 A	3·2	9	G	5·4
5	B 5 A	3·3	4	G 5 K	5·7
5	B 8 A	3·3	32	K	5·8
1	B 9 A	4·4	8	K 2 M	6·4
13	A	3·8	10	K 5 M	6·7
7	A 2 F	4·1	7	Ma	6·8
1	A 3 F	2·6	3	Mb	6·3
4	A 5 F	4·3	1	Md	6·6

It will be seen that there is an almost regular increase in colour from stars of the "Orion type" to stars of the 3rd type, and this shows that there is a close relation between the colour of a star and its spectrum.

Dublin, 1906, Aug. 7. J. Ellard Gore.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Christian Severin.

GENTLEMEN,—

Although the foundations of astronomy were laid by Tycho Brahé, from whose observations, at Hveen, Kepler deduced his laws, the first person to establish an observatory on the principal island of Denmark was Christian Severin, better known under the assumed name Longomontanus, which was derived from his birthplace, the little village of Langberg, near Lemvig, in the amt of Ringkjöbing, where he was born in 1564. He was Tycho's principal assistant at Hveen, and followed him to Prague, returning after his death to Denmark, where he was for two years Rector of the school at Viborg, and in 1605 was appointed Professor of Astronomy at Copenhagen, a post which he held until his own death in 1647. It was in 1641 that he erected the observatory on the top of a tower at Copenhagen, which Römer found so unsuitable that he preferred to make his own observations at Vridlösemagle, a few miles to the west of the city. Severin's tower was destroyed by fire in 1728, and though it was afterwards rebuilt, when the present observatory was erected in 1861, a site was selected somewhat further to the north.

Severin published his 'Astronomia Danica' in 1622, a second edition appeared in 1630, and a third in 1640. It is dedicated to Christian IV., and signed Christianus, Severini filius, Longomontanus. The author, in the second book of this work, describes the three systems of Ptolemy, Copernicus, and Tycho, and gives the preference to the Tychonic, although admitting the rotation of the Earth, as Reymers had done before him. Reymers was accused by Tycho of purloining the idea of the planetary revolutions round the Sun, whilst the latter moved round the Earth, from himself, but, as Mr. Berry remarks in his excellent 'Short History of Astronomy,' "there is no good reason why the idea should not have occurred independently to each astronomer." Delambre suggests that Severin was really a Copernican in disguise, chiefly because he applies the epithet admirable to that system whilst calling Tycho's new. This view seems, however, hardly to be taken by Dr. Dreyer, who, in his recent admirable work on the 'History of the Planetary Systems from Thales to Kepler,' remarks that Longomontanus "rejected the elliptic orbits of Kepler, and his standpoint was altogether that of the sixteenth century."

Severin published other works, particularly an 'Introductio in Theatrum Astronomicum,' which appeared in 1639. He also claimed to have squared the circle, and apparently was not convinced by Pell's refutation of this in 1646. An Appendix to the

'Astronomia Danica' treats of new stars and comets; in the latter part of this he goes off into the astrological significance of the comets of 1607 and 1618.

Yours faithfully,
W. T. LYNN.

Blackheath, 1906, May 11.

Accelerated Motion of the Great Red Spot and Hollow in S. Equatorial Belt on Jupiter.

GENTLEMEN,—

My observations of these objects towards the close of the last opposition were as under:—

1906.	Transit Time. h	m	Longitude System II. °	Telescope used.	Power.
March 24 ..	6	36	31·5	10-in. With.	220
April 7 ..	8	13	30·5	„	„
10 ..	5	43	30·0	„	„
12 ..	7	25	31·6	12½-in. Calver.	315
15 ..	4	57	32·3	10-in. With.	220
19 ..	8	19	34·4	„	„
22 ..	5	42	29·3	„	„
27 ..	4	59	33·7	„	„
29 ..	6	32	29·9	„	„
May 4 ..	5	46	32·2	„	„

The rate, therefore, was as nearly as possible conformable with that of System II. of the Ephemerides, based on a rotation-period of $9^{h}\ 55^{m}\ 40^{s}{\cdot}6$.

On re-observing the planet before sunrise on August 9 I immediately saw that the Red Spot and Hollow were far in advance of their predicted places, and this was corroborated by an observation obtained by the Rev. T. E. R. Phillips on the morning of August 11, and by another made by me on August 16 as follows:—

1906.	Transit Time. h	m	Longitude System II. °	Telescope used.	Power.
August 8 ..	15	6	16·3	10-in. With.	220
10 ..	16	43	15·2	9¼-in. With.	
15 ..	15	52	15·1	10-in. With.	220

So that in little more than three months the markings have lost about $16^{\circ} = 26^{m}\ 29^{s}$ relatively to the zero meridian of System II. In other words, while the rotation between March 24 and May 4 was $9^{h}\ 55^{m}\ 40^{s}{\cdot}6$, it was only $9^{h}\ 55^{m}\ 33^{s}{\cdot}8$ between May 4 and August 8. The latter period is the same as that exhibited by the Red Spot in 1879.

The dark material (extending over about 63° in longitude) forming the S. tropical disturbance on Jupiter must have been in central conjunction with the Red Spot in June last, and seems to have been responsible for the marked increase in velocity shown by the latter, as on several former occasions. But the recent acceleration in the motion of the Red Spot is far greater than has ever been previously observed, and either the Hollow in the S. equatorial belt or the Red Spot appear to have been visible for 75 years (see 'Monthly Notices,' lix. p. 575).

Bishopston, Bristol,
1906, Aug. 16.

Yours faithfully,
W. F. DENNING.

The Total Eclipse of next January.

GENTLEMEN,—

The solar eclipse of next January will be the third of a series of the 18-year period. The first, which occurred on the 22nd of December, 1870, will be ever memorable for Prof. Young's discovery of the reversing layer. There were some dramatic incidents connected with that eclipse: M. Janssen's escape from Paris in a balloon and adventurous aërial voyage to North Africa, only to be disappointed of his object by the state of the weather there; and the wreck of Prof. (now Sir Norman) Lockyer's party on the coast of Sicily in the 'Psyche.' The next return of that eclipse (so to call eclipses of that period) took place on New Year's Day, 1889, about a minimum epoch of solar spots. The shadow-path, which crossed California, was a narrow one, and the greatest duration of totality did not quite equal two minutes, but a large number of photographic records were obtained. And now approaches a third, on the 4th of next January, of which the scene of operations (for such we hope there will be) can only be in Central Asia. The shadow-track will again be narrow, and the greatest duration of totality will not much exceed two seconds.

Apparently the most favourable station for observing it would be Andijan, or Andishan, which is to the north-east of Samarkand and is the terminus of the Russian Central Asiatic Railway. The great Swedish traveller, Dr. Sven Hedin, visited it twice on his famous Tibetan expedition; on the out-journey, after passing from Samarkand to Tashkent, where he had his chronometers regulated at the observatory, he returned southwards as far as Chernyayeva to take the train in its course from Samarkand to Andishan. The route, he says, lay through the fertile valley of Ferghana, amongst gardens, orchards, and fields, sometimes along the banks of the Syr-daria, with hills and mountains to the south, so that no doubt a convenient position could be found for observing. Sven Hedin passed through Andishan again on his return from his Tibetan adventures, when he failed to reach Lhasa. Since his journey a new line of railway has been opened from Orenburg to Tashkent, which renders it, of course, much easier to reach the latter place from Europe. Sven Hedin left Stockholm in June 1899, and returned to it in the same month in 1902, after a journey of three years, which has greatly extended our knowledge of Central Asia. Andishan, in Western Turkestan, is only on the margin of that dreary region, and even in winter it appears feasible to add another to the great eclipse expeditions. Yours faithfully,

Blackheath, 1906, Aug. 9. W. T. LYNN.

Heliacal Risings and Settings of Stars.

GENTLEMEN,—

May I, through the medium of your pages, ask for help in the observation of the heliacal risings and settings of a few standard stars? I am especially anxious to obtain the assistance of observers residing between latitudes N. 24° and N. 40°, as these correspond to the limits within which the astronomers of ancient and classical times lived. I add a list of the stars which I would suggest for observation, together with a very rough indication of the dates near which their risings and settings may be looked for in countries near the middle of the zone mentioned above.

I should like to take this opportunity of correcting a careless mistake which I made on p. 123 of the *Observatory* for 1906 March. I wrote, lines 45 to 47:—"The heliacal rising of the Pleiades heralded the opening of spring some four thousand years ago, whilst the heliacal setting of Orion ushered in the winter." Both statements are wrong. The heliacal rising of the Pleiades heralded the spring equinox in N. lat. 38° about the time when the constellations were designed, nearly six thousand years ago. "Four thousand years ago they marked the position of the Sun at the spring equinox," as I have correctly stated in my 'Astronomy without a Telescope,' p. 102; that is to say, they then were not heliacally rising, but were in conjunction with the Sun.

List of Stars for Heliacal Observation, N. lat. 32°.

Name of Star.	Heliacal Setting.	Heliacal Rising.	Name of Star.	Heliacal Setting.	Heliacal Rising.
γ Pegasi......	March 13	April 6	α Leonis ...	July 25	Sept. 4
α Piscium...	March 29	May 27	β Leonis ...	Sept. 2	Sept. 25
α Arietis ...	April 12	May 9	α Virginis...	Sept. 13	Oct. 29
α Ceti	April 12	June 11	Arcturus ...	Nov. 5	Oct. 22
η Tauri......	May 1	June 9	α Libræ......	Oct. 10	Nov. 21
α Tauri......	May 10	June 20	α Serpentis .	Nov. 17	Nov. 21
β Orionis ...	May 7	July 14	Antares......	Nov. 6	Dec. 16
γ Orionis ...	May 13	July 10	α Herculis...	Dec. 17	Dec. 4
β Tauri.. ...	May 27	June 26	α Ophiuchi .	Dec. 19	Dec. 10
ε Orionis ...	May 13	July 16	σ Sagittarii .	Dec. 11	Jan. 23
α Orionis ...	May 23	July 14	Altair	Jan. 17	Jan. 11
Sirius.........	May 21	Aug. 7	β Capricorni	Jan. 7	Feb. 8
Castor	June 28	July 18	ε Pegasi......	Feb. 7	Feb. 13
Procyon ...	June 13	Aug. 5	α Aquarii ...	Feb. 6	March 3
Pollux	June 30	July 24	α Pegasi ...	Feb. 27	March 9

The above dates are, of course, only approximate indications as to when the phenomenon should be looked for. They will be especially liable to uncertainty in the case of heliacal risings in spring or heliacal settings in autumn.

86 Tyrwhitt Road, St. John's,
Brockley, S.E.
1906, Aug. 22.

Yours faithfully,
E. WALTER MAUNDER.

OBSERVATORIES.

CAMBRIDGE.—The following is a summary of Sir Robert Ball's report for the period 1905, May 1, to 1906, May 18.

Meridian Circle.—The principal work in this department has been the revision and completion for press by Mr. Hartley of a large number of meridian-circle results for the years 1872–1900. This volume will contain the results of all the individual observations from the Cambridge Zone of the 'Astronomische Gesellschaft Catalogue' published in 1897, and in addition the results of about 12,000 observations of stars in the Zone which were not included in the original programme. This work of revision was facilitated by references to the plates of the Astrographic Catalogue taken at Oxford which cover the same part of the sky.

Sheepshanks Equatorial.—Seventy-six plates, each containing four or more exposures, have been made by Mr. Hinks during the year, the greater part of them in continuation of the series for the determination of stellar parallax begun in 1904 by Mr. Hinks and Mr. Russell. The observation of the fainter stars of the working list then adopted is now nearly complete. The observation of the brighter stars was interrupted by the failure of the special colour-screen after a considerable number of plates had been taken. After a number of trials it has been decided that the first kind of screen is not sufficiently permanent to make its further use advisable. A new form of screen has now been devised and constructed experimentally; and it is hoped that within a few weeks the observation of bright stars will be resumed.

Floating Zenith Telescope.—Mr. Cookson has continued the determination of the Constant of Aberration, by Küstner's method, and of the Variation of Latitude. The measurement and reduction of all plates exposed is nearly complete, and a value of the Constant of Aberration depending on the first year's observations will shortly be derived. Mr. Campbell has assisted in this work throughout.

Mr. F. J. M. Stratton (Isaac Newton Student) was appointed an honorary member of the staff in 1905 October. He has been engaged since that time in the determination of the proper motion of faint stars by photography. Mr. H. N. Russell (Carnegie Institution Research Student) gave up his position as honorary member of the staff in 1905 July.

The reduction of the photographs of Eros is proceeding.

The Assistant Director, Mr. H. F. Newall, states that a considerable time was devoted to the preparation for the solar eclipse, which was successfully observed by him at Guelma, and that the Newall Telescope has been used only on a few nights in the year, it having been temporarily adapted for solar observations.

The experience of the winter has shown that, if one may judge by the distinctness with which many of the peculiarities of sun-spot spectra are recorded on the photographs obtained in these unfavourable months, and by the clearness with which the resolution of the spectrum of the umbra into fine lines and bands as recorded by Young, Dunér, Hale, and others has been corroborated on many occasions, the atmospheric conditions at Cambridge for the study of solar phenomena are promising.

Mr. E. T. Whittaker, Trinity College, has continued his investigations on the red region of the spectra of compounds of the alkaline earths, and is completing a set of observations before he leaves Cambridge to take up his new duties at Dunsink.

Mr. J. B. Hubrecht, Christ's College, a research student in astrophysics, has begun work at the observatory, and is preparing to carry out an investigation of the rotation of the Sun by a spectrographic method.

PUBLICATIONS.

Spherical Astronomy*.—There is a class of paper occasionally read at the Astronomical Society productive of some weariness to the ordinary person, which deals with such erudite questions as the difference of results when " Poulkova refractions " are used or otherwise, or which contains obscure references to " Newcomb's equinox " or some other. These things are generally passed over as technicalities, uninteresting to any but the professional astronomer, and it is to make such things plain and clear that Prof. Newcomb has written the valuable and interesting book now before us. It is a book of quite an original type. It is not like Chauvenet's work, in that it does not go into very great minuteness in formulæ and it is rather more elaborate in some respects than Main's. The author has aimed at devising the most expeditious though rigorous method in arriving at results.

A feature of the book is a condensed treatment of the theory of errors of observation and of the method of least squares. These come quite at the beginning along with other instruction in the theory and practice of computation. This is followed by a section on the Fundamental Principles of Spherical Astronomy, which includes an exhaustive enquiry into and development of the formulæ relating to astronomical refraction. The Chapter on

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* A Compendium of Spherical Astronomy, with its Applications to the Determination and Reduction of Positions of the Fixed Stars. By Simon Newcomb. Macmillan & Co., 1906. Price 12*s*. 6*d*.

Precession and Nutation in this part is very enlightening, with historical notes and references. But it is in its remaining sections where this book mostly differs from others. Here are described, with numerical examples, the methods actually used in observatory practice to reduce the position of a star from one epoch to another, to compute star-corrections (which, it may not be known by the general reader, is the generic name for the sum of the effects of precession, nutation, aberration, and proper motion on the place of a star between the beginning of a year and any date in that year), and to find the position of the ecliptic from observations of the Sun and stars. Not only this, but there is an explanation of the principles of combining star catalogues, to form a fundamental catalogue—a process which is generally left to such masters of practical astronomy as Prof. Newcomb and others whose names readily occur to mind. Here the author gives us an Historical Review of observation of stars for position, and the construction of catalogues from the time of Flamsteed to the present day, with comparison of the different methods, which is very interesting reading. Thus he writes about Airy and the Greenwich Transit-Circle:—

> Its construction is an interesting reflection of Airy's methods. He thought out what he might well suppose to be all possible sources of systematic and accidental error, devised means of avoiding or eliminating them, established a complete system of supervision, and then assumed that the results of his system could be regarded as absolute determinations which needed little further investigation so far as possible corrections to their results was concerned. Acting on this system, his transit-circle was not reversible, a quality necessary only when it is assumed that an instrument may give different results if the pivots are turned end for end in their bearings. The circle is not adjustable on the axis because the errors of graduation were determined once for all and were not supposed subject to any further correction . . . .

Then about the German school, by way of comparison:—

> Contemporaneous with the accession of Pond to the Directorship of the Greenwich Observatory was the foundation, by Friedrich Wilhelm Bessel, of the German School of Practical Astronomy. The fundamental idea of this school in the trial of its instrument reverses the maxim of English criminal law. The instrument is indicted as it were for every possible fault, and it is not exonerated till it has proved itself correct at every point. The methods of determining the possible errors of an instrument were developed by Bessel with an ingenuity and precision of geometric methods never before applied to such problems.

A later paragraph tells about Poulkova:—

> Struve's special purpose was to introduce a new era into astronomical determinations by combining on a large scale the qualities of the most refined instruments that art could make with the skill of the most capable observers. It was found that when the latter intelligently devoted the most painstaking attention to the avoidance of every source of error a degree of excellence in their work was reached which was not possible with mere routine observers.

There is a good deal more interesting reading of the same kind which it would not be right to quote, although we should like to do so. The chapter ends with a useful list of catalogues, and is followed by an Appendix of formulæ and tables necessary in computing star-places and catalogues. This book no doubt will find a place in the library of every observatory, and it ought to have a quick sale among astronomers generally.

NOTES.

Gravity in India. — Col. Burrard's memoir (Phil. Trans. vol. 205. pp. 289–318) on the pendulum operations now proceeding in India, referred to on another page of this number, is interesting reading in many ways. One paragraph states that at Calcutta observations were rendered impossible by the ceaseless vibration of the ground, which proved sufficient to cause the pendulums, if left suspended at rest, to oscillate visibly in a few minutes; this effect on the pendulum was produced in whatever plane the latter was swung. But perhaps the most striking fact that is being brought out is this: forty years ago operations of a similar kind but with a different type of apparatus were performed by Officers of the Indian Survey, and the result of these went to show that the force of gravity was relatively small over the whole of India. This remarkable result has formed the bases of theories and controversies, but its accuracy has been questioned. The pendulum experiments now being made show that the earlier results were in error, and that the "negative variation" rests on imperfect data. It is suggested that the fault lay in the flexure of the wooden stand of the older apparatus.

Universal Time.—It has been enacted by the Governor that the Standard Time of the Colony of Seychelles shall be that of the meridian 60° East of the meridian of Greenwich. This has been done after consultation with the Government of Mauritius, where it is expected that the same time will shortly be adopted as Standard. At present there is no other nationality using the zone time four hours fast on Greenwich.

An unexplained Observation.—Under this heading Prof. E. E. Barnard, in *Ast. Nach.* 4106, relates an occurrence of fourteen years ago. On 1892 August 13, at 10 minutes to 5 in the morning Standard Pacific Time, which corresponds to 50 minutes after noon Greenwich Mean Time, he was observing Venus, and in the same field with the planet he saw a star which he estimated to be of the 7th magnitude, but whose place, deduced from that of Venus at the time, does not agree with that of any considerable star in the 'Bonn Durchmusterung.' The observation was made in broad daylight, half an hour before sunrise, and the objects were low down in the sky, and under these circumstances it seems remarkable that a 7th magnitude star should have been seen.

Prof. Barnard thinks it best to put this on record, although he has previously hesitated to do so. He adds that the position excludes the possibility of its being an intra-Mercurial planet, but does not preclude the possibility of its being a planet interior to Venus, though such is not probable.

FROM AN OXFORD NOTE-BOOK.

WE are having some really warm holiday weather. [Indeed today reminds me of a summer day in Washington two years ago, when the temperature in our hotel bedroom was almost unintelligible, until it was discovered that the radiators were nearly red hot, having been started during a "cold snap" some days before by the previous occupant and forgotten.]

It is something to be thankful for that the heat-wave did not come during the Meeting of the British Association at York; for the rooms were well filled, and we might have suffered a good deal. As it was, the temperature in most of them was quite pleasant.

We talked of radium as a source of heat, but even then the discussion did not wax warm; for it so happened that only one side was strongly represented. Lord Kelvin has since written to the *Times* letters which show that if only he had been able to be present the controversy would have been much brisker; but as it was, when Sir George Darwin re-stated his definite opinion that the calculations of Lord Kelvin (and even more so those of the late Prof. P. G. Tait) on the age of the Earth were now so much waste paper, no whisper was heard in deprecation.

A NEATLY worded phrase of Mr. Soddy's is worth setting down. He was arguing that it was now certain, though previously only very probable, that none of the alchemists could have ever effected a transmutation even accidentally, in spite of various apparently well authenticated claims: "for we have realized by this time," said Mr. Soddy, "that there would have been an accompanying development of energy which could not have escaped their attention." Assuming the implication to be that they would have been blown into smithereens, this is at any rate one instance wherein a radium enthusiast cannot be accused of over-stating his case.

A NOTEWORTHY incident of the meeting was the decision to accept an invitation from Winnipeg for 1909; and it is to be further noted that there is, I believe, no Total Eclipse to be observed in that year. Shall we journey by turbine and motor by that time, I wonder, or simply fly?

WHEN the British Association met in Oxford in 1894, Lord Rayleigh made the sensational announcement of the discovery of Argon. Much that has happened in Physics since then has been more or less a consequence of this discovery—rare minerals were examined for gases such as helium; radium was found from the examination of rare minerals; and so on. At the

York meeting Lord Rayleigh sat on the front bench as an interested listener, while his son gave an account of the laborious investigations which had led him to the conclusion that the Earth's crust is only some 30 to 50 miles thick. And Prof. Milne was able to offer, in confirmation of this estimate of the thickness of "Earth's rind," some striking seismological evidence. Hitherto we have been accustomed to find sciences like geology and physics contradicting rather than confirming one another, and the change is pleasant.

We are thus advancing a little beyond the "inch-deep" of Mr. J. R. Lowell (in 'The Footpath'):

They said the fairies tript no more
  And long ago that Pan was dead;
'Twas but that fools preferred to bore
  Earth's rind inch deep for truth instead.

But is not the time come when an actual boring of considerable depth, such as has been suggested already by Mr. C. A. Parsons and others, should be undertaken?

The Stolen Planet: a Scientific Romance, by John Mastin, R.B.A., F.R.M.S. (London: P. Wellby, 1906. Price 3*s.* 6*d.*). "The book is written in the hope that it may prove the means of giving to our youths technical instruction, combined with excitement of a healthy kind, every incident related being based on scientific facts." (Extract from Preface.) We make bold to extract a few of the "incidents related," premising that the story is of a journey through space in an "aërostat," the motive power being "negative force" extracted from the various celestial bodies which came to close quarters.

> The Moon is so familiar to us that we should not gain so very much from landing, I am afraid, and I think we had better leave it. . . . The cold pale Moon was left to pursue her lonely course while we rushed onward with mighty force, the speed attained being so great as to be immeasurable by time and distance, the human mind not having the faculty to realize it. For several days we maintained this headlong rush through space. In silence and solitude the *Regina* travelled on, the wonders of the heavens unfolding themselves before us in ever-changing majesty. Venus and Mercury shone with resplendent beauty, and gradually grew larger till they were like our Moon at the full. The Earth also, with its satellite, could still be clearly seen, and, later, the planet Mars was distinctly visible in the distance. . . . Our powerful telescopes plainly showed him to be a world like our Earth, with the same configuration of land and water. To all appearance, his atmosphere and clouds were identical with those of the Earth, and the spectrum analysis showed this to be a fact, with the air a little richer, the vegetation being red in places. . . . We sped onwards past this radiant world, and very soon crossed the orbit of the planetoids. . . . (pp. 96, 97.)

On the foregoing extract, the following examination questions might be reasonably suggested:—

1. Show by a diagram the configuration of the planets mentioned and indicate the journey of *Regina* among them.
2. From the particulars given of velocity and elapsed time, estimate the approximate value of the solar parallax.

3. Compare the estimates of velocity given above with that given on p. 257 on leaving Jupiter: viz., "so terrific a speed that in the course of a few hours Jupiter *began* to rapidly diminish in *volume*" (italics the examiner's).

4. Find the position of the observer on p. 92, when the Earth was "far away behind us on our left, and now shining on us like a star, and before us the Moon, momentarily growing larger in *volume and density*."

5. What was the Sun doing all this time?

6. In what was "the air [of Mars] a little richer"? Discuss fully the reasons for leaving the Moon.

And we might proceed to ask for a reconciliation of the ideas of the "music of the spheres" indicated by the following extracts:—

> Suddenly there was a frightful blaze of light, yet no sound reached us, owing to the absence of sound waves. . . . Two mighty worlds . . . had suddenly crashed into each other with terrific force. . . . (p. 105.)

> The comet rushed along with a deafening roar, truly frightful and nerve-shaking, especially as it had now grown to such proportions as to occupy the whole of our range of vision. Then came the tail, cracking and exploding like thousands of cannon. (p. 149.)

THERE is a good deal more in the book about the comet: they had to go through the tail, and to make the "electric engines evolve currents of terrific power, in order to neutralize the vast magnetic influence against them" (p. 151), and this produced a "fight to the death between natural and artificial electric forces," which resulted in favour of artifice, the voyagers escaping with no further damage than to find that "the needle had been deflected to the course of the comet." The needle, in fact, did not retain its sense of direction nearly so well as the human beings; for when on p. 192 they encounter a nebula, they find that

> it was quite impossible for us to drive through the nebula *horizontally*, so Fraser caused us to *rise*, and in a few hours we were *above* it and in space, but with no accelerated speed. In fact we were now worse off than before, and at the mercy of anything, for the rising had taken practically all the remaining negative power we possessed, and we had now only just sufficient to enable us to travel in a straight line.

There is much more of the kind in the book, which might obviously be used to good purpose as an educational work, though not exactly as the author intended.

SEVERAL reprints were received the other day from Mr. B. Osgood Peirce, stamped with the precept

> Please do not take the trouble to acknowledge the receipt of this paper.

Whether this is quite a new departure I do not know, but it is the first instance I have come across, and it prompts responsive feelings of gratitude. The desire to have acknowledgments of receipt is natural enough, and in many cases such receipts are no doubt absolutely necessary. But the number of them to be signed and returned is becoming so large as to threaten to be a burden to the smaller observatories, where there is no special librarian; and accordingly those who are able to follow

Mr. Peirce's example may rest assured that their action will meet with approval.

Mrs. Fleming, who was recently made an Honorary Member of the Royal Astronomical Society, has now been appointed Honorary Fellow in the Department of Astronomy at Wellesley College (Mass.). As Mrs. Fleming was born on this side of the Atlantic and has done her work on the other, it is a little doubtful which of these honours has been awarded her by "her own country," but the double event in any case removes the possibility of the traditional reproach. In the number of the 'College News' (July 11) conveying this information we are glad to see further that the Wellesley College Observatory has been enlarged by the generosity of Mrs. Sarah E. Whitin; and the following sentence seems worthy of reprinting:—

> When mildly remonstrated with against putting a very handsome India rug in the students' work-room, Mrs. Whitin replied that having their feet on a good rug would not injure their science!

Mr. Maunder has apparently been relating a thrilling story of his eclipse experiences at North-West River last year. The Expedition on their arrival found the little community of 20 persons very naturally excited by the discovery of some footprints resembling the track left by three bare-footed men with great claws on each foot. One of the "men" must have weighed 700 pounds. The tracks were followed to the lair of the creatures, and it was found that they lived on the numerous voles, and they were actually heard at close quarters, but they were never seen in the open, with one possible exception. This is a much more exciting story than those of the sea-serpent, and I shall hope to read the original account; for at present I have only seen some extracts in the *Star* of August 21, which contained also the following quip:—

> New Milky Way.
>
> Three-inch bold gilt block-letters on the front of a Tottenham milk-cart state "Anylists invited."

In connection with the 'Biographical Directory of American Men of Science,' the industrious Editor (Mr. J. McKeen Cattell) has initiated a scheme for which there can be nothing but praise. It is best described in his own words, which are as follows, the italics being due to the correspondent who kindly drew my attention to the scheme, and who remarks that "a star seems to be the proper way of marking this estimate":—

> *A star is prefixed* to the subject of research in the case of about a thousand of the biographical notes. These are the thousand students of the natural and exact sciences in United States whose work is supposed to be the most important. In each of the twelve principal sciences the names were arranged in the order of merit by ten leading students of the science. The *average positions and the probable errors were then calculated, so that in each science the order of merit was determined together with its validity.*

This licks "Biometry" into a cocked hat; but what is the new *science to be called*? Doxometry?

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OCT 23

EDITED BY

T. LEWIS, SEC.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXIX. OCTOBER, 1906. No. 375.

The Meeting of the Astronomische Gesellschaft.

JENA, the meeting-place of the Astronomische Gesellschaft this year, is on the borders of the Thuringian Forest. A tablet on the hotel where many of the delegates stayed commemorates the fact that Dr. Martinus Luther met the Jena students, and reminds us that we are near the cradle of the reformation. Schiller was for some time Professor of History at the University of Jena, and Weimar, the home of Goethe and Schiller, is within a short distance. These associations, combined with its central position in the heart of Germany, made Jena a very attractive meeting-place. The Optical Works of Carl Zeiss and the Glass Works of Schott added inducements more peculiar to astronomers. With so many attractions, the attendance was a very large one, more than 60 Members being present.

The Meeting began with an informal gathering on the evening of Tuesday, September 11. The first impression made on my mind was the large number of astronomers in Germany. They were not, however, all from Germany, but contingents were furnished from countries as distant as North and South America, Russia, and Scandinavia. The evening was spent in the pleasant occupation of renewing acquaintance with astronomers whom we had met before, and making that of many whom we knew only by name.

The meetings were formally opened on Wednesday morning by addresses of welcome from the Secretary of State, the Pro-Rector of the University, and the Burgomaster of Jena, which were gratefully acknowledged by the President, Prof. Seeliger. After a short interval Prof. Seeliger again took the Chair, being supported on the platform by Messrs. Charlier, Müller, Lehmann Filhés, Weiss, Nyrén, and Dunér. A report was first presented on the state of the Society as to Members &c., and after this reports were made on some of the more important undertakings of the Society. The Zone Catalogues down to $-23°$ are making good progress, that of Strassburg being on the eve of publication. A suggestion was made that the Observatory of Pulkova should undertake the publication of the delayed Dorpat Zones. The compilers of the Variable Star Catalogue had some difficulties because Dr. Wolf found so many new variables. However, he explained that he could not help it, all he did was to expose plates. Three prizes offered by M. Lindemann for determination of cometary orbits were awarded. Dr. Ristenpart reported that all catalogue places as far as 1900 had been entered on his slips for the "Geschichte des Fixsternhimmels."

In the afternoon a visit was paid to the Optical Works of Carl Zeiss & Co., and one could not fail to be impressed by the size and scientific character of these works, which have sprung up in the last 20 years. We saw the manufacture of multitudes of lenses for cameras and microscopes, but some of the most interesting features of the works—particularly the calculations—are hardly suitable for visitors in a holiday mood and were not shown us. The firm owes its origin to the ability and energy of the late Ernst Abbe, whose scientific gifts and business capacity were equalled by his philanthropic regard for the welfare of his employées and his generosity to the town and University of Jena. After visiting the works, we drove or walked to a restaurant on a hill overlooking the town, near to the private observatory where Zeiss' test their instruments. We were entertained at supper by the firm of Zeiss & Co., and afterwards found our way back to Jena by the aid of Chinese lanterns to the tunes of "Gaudeamus igitur" and other student songs.

Thursday began with a visit to Schott's works, where we saw two large discs, 4 feet in diameter, cast. The manufacture of flasks and beakers was being carried on on an immense scale, but perhaps the most interesting sight was the manufacture of glass tubes—some of wide bore, some thermometer-tubes—in lengths of 30 or 40 yards. Among specimens of glass we saw one of heavy flint with a refractive index of 1·9.

The meeting began at 11.30 with an invitation to Vienna for 1908 from Prof. Weiss. A communication was then made by Prof. Seeliger on the empirical terms in the theory of the Inner Planets, which he endeavoured to explain as due to the attraction of diffused matter of approximately ellipsoidal shape surrounding the Sun. Prof. Hartmann gave a description of a machine constructed for him by Zeiss for measuring stellar spectra.

At the afternoon meeting Prof. Förster called attention to the paragraph in the Astronomer Royal's Report to the Board of Visitors relating to the erection of a generating station at Greenwich, and proposed a resolution to the effect that interference with the work of the Greenwich Observatory would be deplored by all astronomers, and expressing the hope that the Government would take steps to prevent as far as possible anything which might interfere with the activity of the Observatory.

An interesting paper was then read by Prof. Albrecht on the application of wireless telegraphy to the determination of the difference of longitude between Potsdam and the Brocken, the station from which the signals were sent being 32 kilometres from Potsdam and 183 from the Brocken. The results were extremely satisfactory. This was followed by a paper by Prof. Ebert on anomalous dispersion, which raised some discussion, in which Prof. Hartmann took the largest share.

In the evening we were hospitably entertained at dinner by the Government of the Grand Duchy of Saxony, in the large room of the Volkshaus, a beautiful gift to the town of Jena by Carl Zeiss & Co.

The next day was spent in an excursion to Weimar, where we visited the houses of Goethe and Schiller, and museums and archives where their memories are piously preserved. As astronomers we were greatly interested in some early maps in the Grand Ducal Library, one of which was made by a son of Columbus, another by Mercator.

My Scottish colleague and I were unable to return to Jena for the meeting on the last day. We took our leave at Weimar with the pleasantest recollections and in the hope of attending the next meeting at Vienna. F. W. D.

The Planets and Planetary Observation.

[Continued from p. 359.]

Mr. Lowell observed Mercury from 1896, Sept. 14 to 26, and says: "The markings on the planet's disc proved to be at once conspicuous. They were easier to see than all the markings on Mars except the most salient ones, such as the so-called seas. So visible were they at all times—for the air had to be distinctly bad to obliterate them—that observations had not been made more than a day or two before the rotation-period of the planet was patent."

Mr. Lowell followed the planet to within 4° of the Sun. His observations were usually made with low powers, viz. 135 to 170 on 24-inch refractor, and 75 to 135 on 6-inch refractor. At moments of best seeing, the 24-inch showed much more than the 6-inch, but on the average there was little difference between the two instruments.

Major Molesworth, at Ceylon, made a number of interesting observations of Mercury in 1898, and I have had an opportunity of seeing his drawings and descriptive notes. The following are a few selections from the latter:—

"1898, April 3,—3^h 15^m Local Time. Appears to be a very white oval spot on S. *p.* limb, with a darker shade round it. Several faint streaks run in from terminator. N. cusp pointed slightly. Image unsteady. 12½-in. Calver, powers 165–230.—$5^h\ 0^m$. Planet steadier and at times quite sharp. Am certain of the existence of a bright spot on S. *p.* limb as seen at $3^h\ 15^m$. A fainter smaller spot on N. *p.* limb. Body of planet very dull, far duller than Venus, but the limb fairly bright. There is a darkish streak just N. of the centre of the terminator extending diagonally to meet the shading near the N. cusp. This streak appeared further west at $5^h\ 30^m$.

"April 5.—$4^h\ 35^m$. Very dark streak extending along the N. limb and just inside the bright region at margin.—$5^h\ 45^m$. Steadier definition. Dark shading at N. cusp. A delicate mottling and veining all over the disc suspected at moments of best definition. Bright spot seen on S. *p.* limb.

"April 9.—$4^h\ 0^m$. Definition very sharp. Very bright spot on S. *p.* limb. P. 340.

"May 23.—$18^h\ 10^m$. Limb sharp and clean-cut, even with high powers. Terminator much shaded. Limb intensely white. Very dark streak running inward from terminator ⅓ of the way from S. cusp. P. 230 and 340.

"May 24.—$17^h\ 50^m$. The shading ⅓ from S. pole is very distinct. Both cusps dull. White spot on N. *f.* limb was very striking and semi-oval in outline, almost like snow-cap on Mars. P. 230.

"May 25.—$18^h\ 0^m$. Limb sharp and white. Same spot on N. *f.* limb unchanged in place and very bright. S. pole cut off by a faint dark streak. Very definite streak extending inwards ⅓ down terminator from S. pole.

"June 1.—$18^h\ 10^m$. Def. wonderfully sharp. Limb bright with two brighter patches near centre. Faint diagonal shade extending from midway between S. cusp and centre nearly to the middle of the limb. Another dark patch nearly at central part of the terminator, and running far inwards to E.N.E. P. 230 and 340.

"June 2.—$18^h\ 0^m$. Apparent concavity of terminator probably an illusion caused by dusky shading-off at its border and protrusion of cusps. Shading on terminator darker just S. of centre. Distinct oblique shade running from a point ⅓ of the way from N. cusp, which can be traced to middle of the disc. Bright patch on N. *f.* limb. P. 230 and 340.

"July 20.—$5^h\ 35^m$. Tint of planet pale white with decided yellowish tinge. Shadings very distinct, especially a dark streak just inside the bright N. cusp and a diffused marking extending in from the centre of terminator. P. 230 and 340.

"July 25 and 26.—To the naked eye Mercury estimated to be twice as bright as Regulus.

"September 20.—$17^h\ 35^m$. Mercury as bright as Procyon. S. cusp protruded and v. bright white, the N. slightly rounded and rather dull. Brilliant oval patch surrounded by faint shade midway between the N. cusp and centre of limb. The latter very bright. Dark streak extends from the terminator inside the S. cusp to near the centre of the limb."

Major Molesworth's observations show that the terminator is much shaded and that a number of dusky streaks and white spots are visible on the disc. The white spots are sometimes very striking on the limb, which is itself pretty bright in variable degree all round. When the Sun had gained considerable power it was found that definition suffered on account of the undue faintness of the image and increase of atmospheric tremors, and in this experience I can quite support Major Molesworth. On the question of the mobility of the markings his observations offer a large amount of evidence, though possibly it is sometimes contradictory: marked changes were occasionally observed even at intervals of ½ or ¾ hour and on successive nights; but were these the effect of variable definition upon delicate features having forms and positions difficult to assign accurately? In several cases the spots exhibited remarkable persistency; thus, there was a bright oval patch on 1898, April 3, on S. *f.* limb, which was also there on 5th, 7th, 9th, and 10th. A bright patch was seen on N. *f.* limb on May 23 to June 17; other markings, apparently of a fairly permanent character, were remarked from time to time. That the phase appears slightly crescented when dichotomy has been reached is correctly explained by Major Molesworth as due to the dusky terminator and brightness of the horns.

In 1900 Mercury was observed by Prof. Barnard with the great 40-inch refractor at the Yerkes Observatory. He found for the diameter at mean distance 6″·59=2965 English miles, and says that "during the observations with the large telescope the disc of Mercury was carefully examined in an endeavour to make out the peculiar linear or canal system recently represented as covering the surface of the planet. No such markings were seen, however. The seeing at times was excellent; especially was this the case on 1900, Aug. 31, when momentary glimpses of the planet showed quite distinctly marked details. There were 3 or 4 large dark spots very much resembling those seen on the Moon with the naked eye. One of these dark markings S. *p.* the centre of Mercury was specially noticeable."

Thus Prof. Barnard fully affirms the existence of definitely observable features on the disc of the planet, but these are not the (probably pseudo) features reported by several other observers.

We may perhaps smile incredulously at the canaliform aspect attributed to Mercury by Mr. Lowell. We may skake our heads doubtfully at the very distinct shadings and bright spots so numerously shown in Mr. Brenner's drawings. We may also regard certain other results very suspiciously, and as by no means proven, but we are bound to accept those of Mr. Barnard. The character of his work in various departments of observational astronomy has been of great value. Often engaging in practical researches of a promising kind, his ability, aided with telescopes of very great power and excellence, enable him to go further than many other observers, and the results he obtains are singularly free from the taint of imagination and illusion.

Mr. MacEwen, of Glasgow, has obtained many observations of Mercury with a 5-inch refractor. Writing in 1895, he described the markings as dusky orange areas and distinct, small brilliant patches, almost points. In 1901–2 he observed the planet on 31 days, and on 11 of these definition was very good. Three distinct markings were seen, and these changed their positions relatively to the terminator. The latter presented a hard and sharply-defined outline. In 1904 April, on four evenings, Mr. MacEwen noted a shaded but fairly obvious area; on two other nights, when the atmospheric conditions were very good, no markings were visible. In June of the same year the planet was similarly devoid of spots.

In 1904 and 1905 Mr. Lepper, of Maritzburg, Natal, examined Mercury on 27 occasions with a 3½-inch refractor; on 1904 June 8 a bright spot was glimpsed in the S. hemisphere about halfway from the equator to the pole and midway between the limb and terminator. A faint dusky streak running N. and S. to the N. of the bright spot was also suspected. On 1904 August 11 and 1905 July 17 there seemed to be a perceptible fading of light along the terminator. Nicks in the latter were observed on several occasions. The colour of the disc usually appeared of a purplish-white tint, with perhaps a greenish tinge at times.

In 1904 April 14 Mr. McHarg, at Lisburn, using a 4·1-inch refractor, detected a dark spot on Mercury, and followed it on five subsequent evenings. He derived the rotation-period of the planet as nearly similar to Schröter and Harding's obtained about a century ago. He gave six drawings in *English Mechanic*, 2043, May 20, 1904.

Prof. Müller of Potsdam concluded, after watching the phases during several years, that they were such as characterised a broken and uneven surface (*Ast. Nach.* 3171).

English weather could hardly ever have been more propitious for observing Mercury than it was at the close of August and early in September 1906. Scarcely a speck of cloud veiled the blue of the sky at Bristol during the 7 days and nights ending Sept. 2. Mercury was at greatest elongation W. on August 29. On August 30, between $16^h\ 45^m$ and $17^h\ 50^m$, the writer observed the planet in a 10-in. reflector, powers 220 and 310. Definition bad at first, but very good during last half-hour of observation, when the higher power could be used effectively. The following details were remarked:—

Terminator dark and very distinctly shaded off.
S. horn dusky and apparently rounded.
Dusky band ⅓ from S. horn to middle of illuminated disc.
Large bright area overlying a considerable region south of centre.

Large dusky spot stretching from terminator between centre of disc and N. horn and curving S.

N. horn very bright and decidedly sharper than S. horn.

Limb bright all round, with suspected differences in intensity forming white spots somewhat similar to those seen on Mars, though far less decided.

On September 1 the planet was examined between 17ʰ and 18ʰ 10ᵐ (sunrise 17ʰ 25ᵐ). The power of 220 was preferred to 310, the gain in definition being considered ample compensation for the smaller disc. Directly the object became really well defined at 17ʰ 25ᵐ, it was noticed that the aspect appeared different to that exhibited on August 30, the markings being now decidedly east of the positions occupied on the former date. The large white patch chiefly in south hemisphere seen on August 30 seemed nearer the S. *f.* limb. There was a dusky band or streak separating the bright spot from south pole. The dark streak on its N. side now showed more prominently. The region N. of the dark streak was occupied by a bright spot which had not been remarked before. The S. horn evidently more blunted and duller than the N., and the terminator looked generally more shaded than on August 30. At times the image of the planet appeared sharply defined. At 18ʰ 10ᵐ the Sun was shining into the tube of the telescope; the planet became faint and definition indifferent.

Some observers have seen the markings very well when the planet has been close to the Sun and at a minimum diameter, but my experiments favour the view that near elongation forms the most favourable period, while the time must be about 2ʰ to 3½ʰ after Mercury has risen, or a similar interval before his setting. When the Sun is high the planet is faint and telescopic performance impaired by the solar rays.

The markings discerned at Bristol in 1882 November I likened to the seas of Mars, and their general aspect certainly suggested resemblance to the Syrtis Major, Acidalium Mare, and other prominent features; but I quite agree with those observers who have thought the Mercurian markings similar to the lunar seas as viewed by the naked eye.

From a number of comparisons of drawings made by various observers, it seems highly probable that certain of the features depicted are very similar if not absolutely identical, and furnish the sort of corroboration required for finding the rotation-period. The best value I can deduce for this period is 24ʰ 42ᵐ, but it cannot be said that the result rests on a strong basis and is thoroughly dependable. A drawing which I made on 1882 November 9, 19ʰ 39ᵐ, presents a suggestive likeness to the aspect of the planet as seen here 1906 September 1. A drawing by Brenner on 1896 July 9, 22ʰ, appears to be of same phase and aspect as one by Major Molesworth on 1898 June 2, 18ʰ 10ᵐ, and there are some further resemblances. But the well-known differences which occur in planetary delineation make it difficult to match such results with certainty. Two figures by different individuals may display lack of uniformity yet represent precisely the same objects. Comparisons of drawings of Mars or Jupiter made at the same times by experienced observers often exhibit very imperfect accordance. Many things conspire to occasion variety. The telescope, the power, the vision, the state of the atmosphere, the method of delineation, &c., are different in every instance, and instead of being surprised at the disagreement revealed in such cases we ought perhaps to regard the fairly conformable pictures sometimes made as highly satisfactory.

[To be continued.]

Where did the Nebulæ get their Helium?

There can be no reasonable doubt that helium is a product of radio-activity. Its evolution, indeed, is the one assured permanent outcome of the series of changes to which radium is subject. Another, of a totally different kind, is associated with it, but only by a sagacious conjecture. Radium, when it has reached its fifth and final stage of degradation in energy, is thought by Prof. Rutherford to be probably identical with lead. And each drop to a lower level is accompanied by the emission, from the disintegrating atom, of an "alpha-particle," commonly held to be a helium-atom just out of the shell. If this be so, radium is made up of lead and helium, or, rather, of the constituent electrons of lead and helium. For it is not what is ordinarily understood by a compound. About one million times more energy is locked up in a gram of radium than is set free by the union of hydrogen and oxygen to form a gram of water. Moreover, radium has all the distinctive properties of a so-called "element." It finds a suitable place in Mendeléef's periodical table; its chemical and spectral relations are entirely normal; there are, in fact, no elements unless radium be one. In what sense, then, is it said to consist of helium and lead? On the electronic theory of matter, each atom is a self-subsistent system. Several such entities, each preserving its integrity, may conceivably be grouped together to form compound molecules, but it is incredible that a truly elementary atom, like that of radium, should be an aggregate of at least six subordinate, self-centred systems. Helium must accordingly be regarded as existing only potentially in radium. Its atoms take shape at the moment of expulsion. They owe their origin to a re-arrangement of electrons attending the cataclysm certified by their projection.

The identity, it is true, of helium-atoms with alpha-particles has not been directly proved. The question is, to some extent, in suspense. What is actually observed to take place is this. Radium gives off spontaneously a heavy, inert gas known as its "emanation." This also is radio-active, and therefore unstable and evanescent. As it decays, helium develops. At the end of four days, the effectiveness of the emanation is reduced by one-half, and the complete spectrum of "clevite-gas," marvellously ordered in a sextuple series, emerges on the transmission of an electric current through the enclosing tube. This is a strictly irreversible process *. It involves the expenditure of an enormous quantity of energy. The fall of Humpty Dumpty was a reparable catastrophe compared with that eventuating in the construction of a helium-atom. By no possible effort of nature, apart from conditions unimaginable by us, can radium be built up again out of helium and lead. Its materials, once dissevered, must remain so. Their redintegration would defy the powers of an army of "drudging goblins" working with a will from nightfall to cockcrow.

We must then recognize that, while helium is apparently evolved in the course of the atomic break-up of radium, radium cannot possibly, to our apprehension, have been fabricated by the forcible union of helium with lead, or any other substance or substances. Helium is a derived, not an aboriginal element. Two separate and independent modes of origin cannot rationally be attributed to it. Its terrestrial distribution is corroborative of this conclusion. It invariably pervades radio-active minerals, and probably none beside †. The facts of its occurrence are uniformly consistent with the view of its being a by-product in the great elemental laboratory. Helium pre-supposes radium, or some similar generating metal; and this must be the case in every part of the universe. There could be no science of astrophysics unless the various species of matter retained their individuality throughout the length, breadth, and depth of space.

Now helium is an ordinary, if not an unfailing, constituent of gaseous nebulæ. Its distinctive yellow ray (D_3) was detected by the late Dr. Copeland, December 28, 1886, in the spectrum of their great representative in Orion; and Prof. Campbell has observed two additional members of the same series of much shorter wave-lengths, which are met with as well in several planetary nebulæ. "Green" helium (formerly called "parhelium") makes no show, or a very poor one, in nebular light. But what immediately concerns us now is, not so much the particular state of helium in nebulæ, as the undoubted, though surprising, fact of its being there at all. For its presence carries certain implications with it. The substance is unlike every other in having known antecedents. In this one case, science, astonished and half-incredulous, is enabled to superintend the production of a new and peculiar kind of matter.

Clearly, however, matter so originating cannot make part of

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* W. Meigen, *Nature*, Feb. 22, 1906.

† Its association with platinum does not, in Mr. Soddy's opinion, constitute a genuine exception to this rule. See *Nature*, Dec. 14, 1905.

highly primitive cosmic formations. Its existence asserts an earlier condition, in which over-elaborated atoms crumbled into ruin while surrendering vast stores of previously accumulated energy. What follows? Apparently this. The place of nebulæ is not at the root of the genealogical tree of the universe. Viewed from the standpoint of radio-activity, they present themselves to us, no longer as scenes of incipient growth, but rather as effete remnants bearing witness to the slow process of decay. We see in them the cast-off exuviæ, not the seed-plots of suns. At the same time, this inversion of ideas is not instantly and absolutely demanded of us. It is, so to speak, prospective. If what we seem to know about the relations of the chemical elements be verified, it will have to be made; but there are still contingencies ahead. The science of radio-activity has not yet outgrown its nascent stage. Its principles must approve themselves to a longer and fuller experience before they can be implicitly accepted. And a recent paper by Prof. J. J. Thomson on the number of electrons in atoms * gives a significant warning of their being subject to revision.

The helium difficulty, however, is not the only one that besets the nebular hypothesis of the genesis of suns. Grave misgivings as to its truth have been raised by the detailed study of nebular structure. "For several years," Dr. See wrote in 1902, "the impression has been growing on the minds of men of science that perhaps nebulosity may be expelled from the stars, and does not really afford the basis out of which they are constituted" †. Diffuse and irregular nebulæ do indeed present an aspect wholly irreconcilable with the opinion that their particles are being collected by their mutual gravitation to form sun-like bodies. They appear to be dominated by forces of a widely different character—by forces that rend and perforate, that draw out floccules into filaments, that project matter into wings and huddle it into wisps. Planetary nebulæ, too, present features importing less obviously, though perhaps even more surely, a similar conclusion. An object of the kind, seen under the best conditions, is perceived to consist typically of a central star surrounded by two or three concentric envelopes. The analogy of the entire construction to the head of a great comet near perihelion is unmistakable, and intimates a conformable method of production. From the nuclear star—the organising centre—successive floods of subtle material presumably issued at intervals perhaps widely separated; each found its own limit of diffusion where attraction balanced repulsion; and the combined visual effect resulted of superposed, dimly luminous discs. Further, the discs take the shape of rings when the expelled shining "fluid" collects into a shell near the surface of its sphere

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* *Philosophical Magazine*, June 1906.

† *Popular Astronomy*, No. 100.

of diffusion. Photographic evidence corroborates these inferences. Prof. Barnard regards the facts registered on his plates as wholly subversive of the nebular theory of star-development *. Enunciated on the strength of imperfect telescopic views, it collapses before the disclosures of the camera. The branched and straggling nebulæ wonderfully depicted by it are, indeed, certainly connected with the stars they involve, but the association may be of an opposite kind to that assumed by Sir William Herschel. These stars invariably belong (so far as is yet known) to the helium type. They are brilliantly luminous objects, most likely at the acme of radio-activity, and all aglow with liberated atomic energy. They certainly possess ample stores of disintegration-products, which they may very well have shared with the nebulous appendages developed by their internal explosive forces. This, at least, is one possible source of nebular helium.

AGNES M. CLERKE.

A Fine Summer.

As early as March 6 and 7, on each of which days the Greenwich maximum thermometer surpassed by more than 4 degrees the previous highest for the day, the fact being further emphasized on the 7th by a "record" sunshine-register of 94 per cent., we had a foretaste of what might happen later in the year. But though certain other days, notably April 12 and 13, seemed to recall the promise of those early spring days, yet the prevailing impression through most of the summer was that of a fine, dry, and sunny, but not remarkably warm season; and in May even the sunshine was far below the average.

In July, however, for the first time since January, the mean temperature for the month was above the average, and later still the conditions became so abnormal as to attract universal attention. The average number of days when the Greenwich maximum exceeds 80° is fourteen. This year until the middle of August there were eleven such days, not more than two in succession at any time. A week later three such days occurred, on the 21st, 22nd, and 23rd, the 22nd being uncommonly warm, thus bringing the total to fourteen, the exact average. Cooler weather followed, until on the 29th the morning minimum reached 44°·1, the lowest reading since June. But all was not over, for on the 30th the temperature again passed 80° and with the 31st came the highest reading for the year, 94°·3, on a cloudless day with no apparent prospect of an immediate change in the weather. This range of over 50° in less than three days is a record for Greenwich, where the monthly range only reaches 50° once in three years on an average. In sixty-five years there had been only two days,

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* *Astrophysical Journal*, vol. xxiii. p. 146.

1868 September 7 and 1898 September 8, when the Greenwich maximum had reached 90° later in the year than August 25, and only four days, 1858 June 16 (94°·5), 1868 July 22 (96°·6), 1881 July 15 (97°·1), and 1893 August 18 (95°·1), on which a higher reading than 94°·3 had been reached. In addition to this the absence of cloud ensured a daily minimum 56°·1, not abnormally high, thereby also assuring a daily range far beyond the average, so that it was much less trying than, for instance, 1893 August 18, above referred to, when the minimum was 66°·8.

It would be perhaps wearisome to give full details of the many "records" that were broken during the unusually permanent anticyclone that was then establised in this part of the world; but, until light clouds again appeared on September 3, the only apparent variation was the direction of the light breeze; if that were easterly the maximum was rather lower, so that the readings on September 1, 2, and 3 were respectively 91°·9, 93°·5 (the highest ever recorded here in September), and 91°·0.

Only on three previous occasions had so many as three consecutive days produced maxima above 90°, 1868 July 20–22, 1876 August 13–15, and 1893 August 16–18, so that in this respect 1906 has set up an absolute record of four such consecutive days.

There are some interesting features about this run of four consecutive "record" maxima. It is unusual for any year to produce a long succession of days, each of which is a "record" for the day. The last occasion was eleven years ago, the year 1895 having the exceptional number of eight such days, September 23–30, each of which registered the highest maximum for the day; incidentally 1895 also had six consecutive days, February 5–10, each of which registered the lowest minimum for the day. Time will show whether this feature will also be repeated in 1906. One other peculiarity in connection with September 2: 1906 holds the record for the month as before noted, but for the actual day, 1906 holds the record by no less than eleven degrees, a distinction which is by no means common. The following is a list of such days: 1859 April 6 (11°·5), April 7 (11°·7), 1869 April 14 (12°·2), 1868 May 3 (10°·8), 1895 September 24 (11°·3), 1906 September 2 (11°·0). No other day in the series holds record by so much as nine degrees at the upper end of the scale. A discussion of minimum temperatures would be out of place just now.

The break in the weather that followed was not so violent as might have been expected, the temperature during the latter half of September being not far from normal.

As regards the sunshine which has been so marked a feature of 1906, even when the weather has not been hot, except during May, the monthly register of sunshine has been well above the average. The duration measured in the three "summer" months is not quite equal to that of 1899, the actual figures being for

1899 756·4 hours, and for 1906 752·4 hours, the next best being 1887 with 715·0 hours, and 1901 with 691·0 hours. The inclusion of May, in which 1906 did not excel, revises the order; 1899 still leads with 960·4 hours, followed by 1901 with 928·1 hours, 1906 with 905·4 hours, and 1897 (not 1887) with 902·4 hours. Including April again modifies matters, since 1899 was then deficient, the first four being 1901 with 1129·4 hours, 1906 with 1123·9 hours, 1899 with 1063·7 hours, and 1897 with 1047·1 hours. For the six months April to September inclusive, 1906 easily holds first place, with a total register of 1309·6 hours, the next three being 1901 with 1251·1 hours, 1899 with 1221·2 hours, and 1897 with 1161·6 hours. W. W. B.

The Sun-spots of 1906.—III.

The chief features of the sun-spot record for 1906 July were:—

1. At the beginning of the month a great procession in the northern hemisphere of groups of moderate size.
2. A quiet time from July 6 to July 16.
3. In the third week of the month two small processions, both in the northern hemisphere, one of them near the equator.
4. In the fourth week a long procession of groups of moderate size immediately north of the equator.
5. Sudden formation of a great group in the southern hemisphere on July 28.

On the whole July has been, so far, the most active month in the present year.

Rotation 706 commenced July $5^{d}\cdot02$.

Notes on the Principal Spot-Groups.

Group 5903.—Large regular spot, area about 150 millionths of the Sun's visible hemisphere, followed by a short train. Total area of group about 200. Formed suddenly on July 3.

Group 5904.—Regular spot, area about 70. Formed suddenly.

Group 5905.—Long scattered stream, which formed suddenly in advance of Group 5903, and north of Group 5904. Area about 100.

Group 5908.—A spot of area about 120, preceded by a number of small unstable spots.

Groups 5912, 5917, and 5921 formed a short procession; total area about 180.

Groups 5913, 5915, and 5920 formed a short procession; total area about 180.

Spot-Groups in 1906 July.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	d	°	°	
5903	July 3	July 12	10	July 6·4	342	+21	Regular with train.
4	4	10	7	5·4	355	+11	Short stream.
5	4	10	7	5·3	356	+18	Long sparse stream.
6	5	5	1	5·0	0	+ 3	Two small spots.
7	6	9	4	10·8	283	+ 5	Small spot.
8	6	15	9	10·3	290	+18	Stream. Much change.
9	8	10	3	12·1	266	+22	Small stream.
5910	9	10	2	6·4	342	−14	Few small spots.
1	10	12	3	12·3	263	−10	Short stream.
2	11	22	12	17·1	200	+18	Large, regular, and train.
3	12	24	13	18·7	179	+ 7	Regular spot.
4	14	18	5	12·9	256	+ 2	Few small spots.
5	14	18	5	18·2	186	+ 7	Few very small spots.
6	14	15	2	19·5	169	−19	Few small spots.
7	15	19	5	17·6	193	+18	Few small spots.
8	15	23	9	18·6	180	−12	Long sparse stream.
9	16	17	2	16·5	208	−15	Few small unstable spots.
5920	16	19	4	17·5	195	+ 6	Compact cluster.
1	16	20	5	18·2	185	+16	Short irregular stream.
2	17	24	8	22·2	133	+ 8	**Return.** Irreg. cluster.
3	18	29	12	24·4	104	+ 4	Compact irregular stream.
4	19	30	12	24·9	97	+ 3	Large double spot.
5	20	21	2	19·1	174	−22	Stream.
6	21	27	7	26·6	75	+ 7	**Return.** Small spots.
7	23	23	1	27·3	66	−23	Small spot.
8	23	30	8	28·5	49	+ 8	**Return.** Small spot.
9	23	25	3	28·9	44	−11	**Return.** Small spot.
5930	24	29	6	26·6	74	−11	Sparse stream.
1	25	Aug. 6	13	Aug. 1·1	1	+22	**Return.** Regular.
2	27	July 28	2	July 30·3	25	− 2	Two small spots.
3	28	Aug. 5	9	30·3	25	−16	Sudden outburst.
4	30	July 30	1	29·0	43	+14	Small stream.

Group 5922.—Third apparition. Return of Group 5889. Final stage.

Group 5923.—A pair of spots with a number of small companions; total area about 200.

Group 5924.—A large double spot, area about 100, close behind Group 5923.

Group 5926.—Return of Group 5893. Final stage.

Group 5928.—Return of Group 5894. Final stage.

Groups 5923, 5924, 5926, and 5928 formed a long procession.

Group 5929.—Return of Group 5895, much diminished. Final stage.

Group 5931.—Third apparition. Return of Group 5898 in a diminished form. Large regular spot, area about 300.

Group 5933.—Sudden outburst. Large composite spot, of area about 1200 by the end of the month.

E. Walter Maunder.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Total Eclipse of next January.

Gentlemen,—

The statement by Mr. Lynn in the September number that the coming eclipse will be the third of a series in the 18-year period is liable to misconception. If meant to be taken literally it is incorrect, for this eclipse is in reality drawing near its end. It began its career as a central eclipse (annular in the antarctic regions) 1059 August 11, and each 18-year period it has drawn slowly northward. The eclipse of 1870 was not even the first totality of the series observed in Europe, for the eclipse of 1816 was observed in Germany. Total eclipses of the series also crossed Europe in 1654 (British Isles), 1708, 1762. In 1925 the eclipse will be total at Boston, U.S.A., in 1961 in Italy, &c. In 2033 it will return for the last time as a total eclipse (Alaska and Arctic Ocean).

The duration of totality in the coming eclipse is given by Mr. Lynn as "two seconds": this is evidently a misprint for "two minutes." The following is a list of durations of totality on the central line next January:—Long. 42° E., $1^{m}\ 12^{s}$; 60° E., $1^{m}\ 42^{s}$; 75° E., $2^{m}\ 12^{s}$; 88° E., $2^{m}\ 24^{s}$; 101° E., $2^{m}\ 6^{s}$; 117° E., $1^{m}\ 36^{s}$; 131° E., $1^{m}\ 12^{s}$.

Yours faithfully,

A. C. D. Crommelin.

Royal Observatory, Greenwich,
1906, Sept. 27.

John Gray.

Gentlemen,—

Johnson tells us, in his account of Thomson in his well-known 'Lives of the Poets,' that he derived the scientific information on which he founded his poem in memory of Sir Isaac Newton (published in 1727, the year of the great astronomer's death, in which also appeared "Summer," the third of the 'Seasons' in order of publication) from Mr. Gray, whose instructions enabled him "to perform as an exact philosopher." This refers to John Gray, who, in conjunction with Andrew Reid, brought out in 1733 an abridgment of the 'Philosophical Transactions' from

1720 to 1733. Gray died in 1769, and in the *Gentleman's Magazine* for that year his death is mentioned under the date of July 17, as that of "John Gray, Esq., F.R.S., well known to the learned world." Five years before his death, *i.e.* in 1764, Gray was elected Rector of Marischal College, Aberdeen, and the copy of his Abridgment of the Transactions which is at the British Museum was presented by the University of Aberdeen in 1897. Although it is called the sixth volume, I cannot learn that any other was issued. It is bound in two volumes, of which the first (called Part I.) contains the Mathematical Papers, and the second (called Parts II., III., and IV.) contains the Physiological, Anatomical and Medical, and Philological and Miscellaneous Papers respectively. The first Part is divided into chapters, the fourth of which is on Astronomy. The sixth of the papers in that chapter is on "a new discover'd motion of the fixed stars," *i. e.* it is Bradley's account of his discovery of aberration, sent to Halley in December 1728. What renders this Abridgment of permanent interest is that it bears the imprimatur of thirteen Fellows of the Society (including the names of Halley, then Astronomer Royal, and Bradley, then Savilian Professor of Astronomy at Oxford), who state that it "being begun by our approbation and encouraged by the assistance of many members of the Royal Society, who have revised and improved their own papers; we believe the same, from our knowledge of the abilities of the Compilers, to be well executed."

I cannot find that John Gray wrote or published anything else.

Yours faithfully,

Blackheath, 1906, Aug. 2. W. T. LYNN.

The Brightness of Mercury.

GENTLEMEN,—

I have read with much pleasure, in the September *Observatory*, the instructive paper upon Mercury by that veteran observer, Mr. Denning. The subject is so fully treated that it is difficult to add anything, but I venture to send a few notes, based upon calculations, most of which were made some time ago by means of Halley's formula.

The orbit of Mercury is so excentric that his theoretical brightness, when at his greatest brilliancy for the Earth, varies considerably. Let L stand for maximum brilliancy. If we call this at mean distance 100, then at perihelion $L=159$, and at aphelion $L=74$. Thus the planet at perihelion is more than twice as bright as at aphelion, the Earth being so placed as to secure maximum effect in each case.

If Mercury and Venus were equal in size and albedo, the former at maximum perihelion brilliancy would be about 2·3 times brighter than the latter when at her maximum brilliancy. The superior size and albedo of Venus completely turn the scale, with the result that Venus at her brightest is about 12 times brighter than Mercury at his brightest. This is on the assumption that the albedo of Venus is to that of Mercury as 4 to 1, perhaps too low an estimate. If we assume Mercury to be at mean distance from the Sun, and that the albedo ratio is 5½ to 1, then Venus will be about 25 times brighter than Mercury, both planets being supposed to be in position for maximum brilliancy in relation to the Earth.

Of course the foregoing results are all theoretical, and the observed brilliancy of both planets will be affected by the intensity of twilight and by their apparent distance from the Sun. Mercury especially will suffer from proximity to the Sun, so that the position in which he is theoretically brightest may differ considerably from that in which he actually appears brightest to us.

It is interesting to note that, theoretically, the excentricity of Mercury's orbit allows the planet at maximum brightness to be, (*a*) at greatest elongation east or west, (*b*) between superior conjunction and greatest elongation, (*c*) between inferior conjunction and greatest elongation. When at perihelion, Mercury's elongation from the Sun for maximum brilliancy is only about 15°, and this is a serious drawback; the greatest elongation for perihelion is about 18°.

One point, referred to by Mr. Denning, but left out of account in the foregoing calculations, is the effect of the variation in the angle between the rays incident and those reflected. In what way, and to what extent, does this alter the intensity of the illumination?

To put the matter as simply as possible: suppose half of a rough sphere, P, to be lighted by parallel rays from a distant source, S, S and P being fixed. About P, as a centre, let a body M revolve in a circle, the distance MP being such that rays, reflected to M from P, may be considered parallel. Let SMP be always in one and the same plane.

Viewed from M, the sphere P will exhibit phase-changes as M revolves. The brightness of P will depend, partly upon the amount of illuminated area visible from M, and partly upon the angle between the incident rays, parallel to SP, and the reflected rays, parallel to PM.

The illuminated area is easily calculated by means of the angle SPM; but I do not know how to estimate the variation in reflective power, which is—so to speak—independent of the area, but is connected with the variation of the same angle. The subject is further complicated by the fact that, unless the surface is uniform, different parts of P may have different albedoes even for incident and reflected rays, which make equal angles at those parts.

In the case of the Moon, it appears that the brightness increases as the angle SPM diminishes, so that the full Moon is more than twice as bright as the half-Moon.

I shall be glad of information upon this difficult point, which is not dealt with in ordinary English works on optics.

Dr. Spitta's able paper—on Jupiter's Satellites, and the light reflected from an unpolished sphere (M. N. R. A. S. vol. xlviii. pp. 32–48)—deals with certain aspects of the subject. I believe that the problem is considered in some German works upon photometry, but I do not know the conclusions arrived at.

For additional results, based upon Halley's formula, reference may be made to the 'Journal of the British Astronomical Association,' vol. x. pp. 296–300.

Yours faithfully,

Leeds, 1906, Sept. 15. C. T. WHITMELL.

"*An unexplained Observation.*"

GENTLEMEN,—

With reference to the above note on p. 368 of your last number, the position of Venus on August 13, 1892, was, according to *Whitaker's Almanac*, R.A. $6^h\ 52^m\ 40^s$, Dec. $+17°\ 12'$. This place is close to (perhaps within) the border of the Milky Way in Gemini: we know that new stars, almost without exception, have appeared in or near the Milky Way; is it not possible that what Prof. Barnard saw was a Nova? If so, an examination of photographic plates taken later in that year might be advisable.

Yours faithfully,

Harewood Lodge, Meltham,
1906, Sept. 5. CHARLES L. BROOK.

Dr. Halley's Arms.

GENTLEMEN,—

Having raised in your issue for March, 1906, the question of Dr. Halley's ancestral origin, it may be in order now to quote briefly from a letter recently received from a London correspondent:—

". . . . The results of my enquiry at the College of Arms have been disappointing. It appears that no grant of arms has at any time been made to a person of the name of Halley, and that no Halley pedigree is recorded at the College. I am still to learn whose were the arms borne (according to Aubrey) by Edmond Halley. . . ."

In view of the above statement, John Aubrey's ascription of the coat, "Sable, a fret and a canton argent," to Dr. Halley is exceedingly curious. In this connection, consult *Notes and Queries*, London, tenth series, vol. v. p. 406, and the *Devon Notes and Queries*, Exeter, vol. iv. (July 1906) pp. 86–88. Some items of new biographical material concerning Halley are included in a rather extensive series of "Extracts from British Archives" now being published in the *Magazine of History*, New York, 1906.

Chicago, Illinois, U.S.A.
1906, Sept. 14. EUGENE FAIRFIELD MCPIKE.

The Duplicity of Mizar.

GENTLEMEN,—

In the monumental work of Mr. Lewis on double stars and their observations by Struve and others, which has recently been published as the fifty-sixth volume of the 'Memoirs of the Royal Astronomical Society,' and will undoubtedly form the standard of reference to the subject for many years to come, it is stated that the duplicity of ζ Ursæ Majoris had been noticed by Riccioli so early as 1650, and the same statement is made in Miss Clerke's 'System of the Stars' (second edition, p. 150). In Webb's 'Celestial Objects' this is expressed in a more doubtful manner.

In the course of a somewhat careful search through Riccioli's works I have failed to find a record of any such observation. In his 'Almagest' he speaks of the star (Alcor) visible to the naked eye near Mizar, and quotes from Apian an Arab proverb about it, that a person who wilfully cannot see a large or evident object though he can a small one not so clear, may be said to see Alcor and not to be able to see the full Moon. I think many persons when they are first told that ζ Ursæ Majoris is a double star fancy they have seen it so with the naked eye, because they know they have seen Alcor, which is 11½ minutes distant from it, and the same mistake may have been made in interpreting Riccioli. That careful writer Houzeau avoids, in the 'Vade Mecum,' making the above statement, which he must have seen in Webb or some other book, and only says of Riccioli that he mentions as test-objects two very close pairs in Capricornus and in the Hyades respectively, in each of which the components were 3′ or 4′ distant from each other. The two stars in ζ Ursæ Majoris are about 14″ apart, and it may be doubted whether Riccioli had a telescope capable of separating them. Hooke, indeed, noticed the duplicity of γ Arietis in 1664 (the components of which are about 8″ apart), but his attention was specially drawn to that star in consequence of the comet which appeared near the end of that year passing near it.

If I am wrong with regard to Riccioli, I should be glad to be referred to the passage in which he mentions the duplicity. There is no doubt that Bradley was the first to determine the actual distance between the two stars. Yours faithfully,

Blackheath, 1906, Sept. 7. W. T. LYNN.

Comet I. 1793.

GENTLEMEN,—

In No. 4117 of the *Astronomische Nachrichten* Dr. Holetschek points out that in Cooper's 'Cometic Orbits' the comet which was independently discovered by Caroline Herschel on October 7, 1793, is erroneously called II. instead of I. in that year. It was first discovered by Messier on September 27, and passed its perihelion on November 5, fifteen days before the comet detected by Perny on September 24. In Mr. Chambers's valuable 'Catalogue of Comets' the same mistake occurs, and, in the note on p. 523 ('Handbook,' vol. i.), the reference 134 should be 133. This is the comet the orbit of which was calculated by Bouchart de Saron whilst in prison; he was guillotined on the 20th of April, 1794, during the Reign of Terror (commonly called by the French simply "la Terreur") for protesting against the suppression some years before of the parliament of Paris, of which he was President.

Yours faithfully,

Blackheath, 1906, Sept. 17. W. T. LYNN.

OBSERVATORIES.

THE following notes on work at various Continental Observatories are extracted from the 'Vierteljahrsschrift der Astronomischen Gesellschaft,' and refer to the work during the year 1905.

ARCETRI. *A. Abetti.*—Small planets and comets with the 11-inch equatorial. With the transit-circle the stars in the Catania photographic zone (-1° to $+11^\circ$) have been observed.

BAMBERG. *Ernst Hartwig.*—The 6-inch refractor and the heliometer have been principally used for observation of variables. In addition, Mösting A, diameters of Venus, and the cusps in the solar eclipse of 1905 August 30 have been observed with the heliometer.

BERLIN. *Hermann Struve.*—Re-observation of the A.G. Zone $+70^\circ$ to $+75^\circ$, and of occultation stars for Dr. Battermann. Time-service.

BERLIN (Astronomisches Rechen-Institut). *J. Bauschinger.*—Computation of star ephemerides for Southern Hemisphere, viz., 303 mean places and apparent places for 123 stars. Ephemerides for small planets. Dr. Clemens has made a number of measures of the Moon, principally in the phase from Full to New.

BONN. *F. Küstner.*—The 30-cm. refractor has been employed in continuing the photographs of stars of Type II. Also of α Arietis, α Persei, α Tauri, α Canis Minoris, β Geminorum, and α Boötis, stars near the ecliptic, for estimation of solar parallax. The work on the Bonn A.G. Zone ($+40^\circ$ to $+50^\circ$) has been progressing. Time-service.

BOTHCAMP. *Paul Guthnick.*—The Zollner photometer was in use for continuing the observations for brightness of Jupiter's Satellites, a large number of estimations being secured. Similar work is in progress on Enceladus, Tethys, Dione, Rhea, Titan, and Iapetus, Nova Aquilæ, β Persei, and λ Tauri. With the 11-inch refractor Comet 1905 *b* was observed, and drawings made of Venus and Mars.

BRESLAU. *J. Franz.*—The work of measuring the negatives on glass for co-ordinates on the Moon's surface was in progress. The transit was used for Moon-culminators and 'Jahrbuch' stars.

DÜSSELDORF. *Wilhelm Luther.*—A large number of observations of small planets. Up to the end of 1905 the total number of observations made here by Brünnow and Luther reached 2769. The Merz objective is 164 mm. Ephemerides for small planets.

[To be continued.]

BIDSTON, LIVERPOOL.—Mr. W. E. Plummer's report for 1905 is very like its predecessors. The number of transits observed for time determination was rather in excess of that of the previous year, but the number of comets observed with the equatorial was rather less. The measurement of the photograph of the cluster in Hercules lent by the Yerkes Observatory was completed and the results communicated to the *Monthly Notices*, vol. lxv. The meteorological and seismological work proceeds, and in connection with the latter it is noted that many earthquakes recorded by the exploring party in the Antarctic Expedition were registered on the instrument at Bidston, although intermediate stations did not, in all instances, record the tremor.

WE have also received reports for last year from the Government Observatories in the Colonies of Mauritius and the Transvaal, which point in both cases to the energy of the Director and his staff, but since they deal with magnetism and meteorology, they call for little notice here. At Mauritius, as is well-known, solar photographs have been taken for some years, so that this may be considered partly an astronomical observatory. In Mr. Innes' report there is a remark to the effect that several requests have been received from public bodies that the Observatory should supply exact Standard Time (2 hours fast on Greenwich), but at present the Transvaal Government has not found it possible to supply the necessary funds. So that here the title does not yet apply.

PUBLICATIONS.

DR. SEE'S RESEARCHES ON THE PHYSICAL CONSTITUTION OF THE HEAVENLY BODIES *.—If it be assumed that the Sun consists of spherical layers, the density σ being a function of the radius x, then if $4\pi\sigma_0\mu$ denote the mass enclosed within a distance x of the centre,

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* *Astronomische Nachrichten,* No. 4053.

$$\mu = \int_0^x \frac{\sigma}{\sigma_0} x^2 dx \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

σ_0 being the value of σ when $x = 0$.

Again, if there is equilibrium under the pressure and mutual attractions,

$$-\frac{dp}{dx} = \sigma\gamma \frac{\mu}{x^2},$$

where γ is the constant of gravitation.

If, further, we may assume that $p \propto \sigma^k$, we have, on integrating the last equation,

$$1 - \left(\frac{\sigma}{\sigma_0}\right)^{k-1} \propto \int_0^x \frac{\mu dx}{x^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

We may replace the sign $\propto$ by $=$ if we choose the units suitably.

These equations (1) and (2) were first handled by Mr. J. Homer Lane in 1869; he used the value $k = 1{\cdot}4$. Dr. See has lately (*A. N.* 4053) resolved them for the value $k = \frac{5}{3}$, appropriate to monatomic gas.

We quote the first few terms of Dr. See's solution in ascending powers of x^2:—

$$\mu = \frac{x^3}{3} - \frac{x^5}{20} + \frac{x^7}{240},$$

$$1 - \left(\frac{\sigma}{\sigma_0}\right)^{\frac{2}{3}} = \frac{x^2}{6} - \frac{x^4}{80} + \frac{x^6}{1440}.$$

It is easy to verify that this is, in fact, a solution to the order written.

We next have to enquire what value of x makes $\sigma = 0$; this is known technically as putting in the boundary conditions; it gives the radius of the Sun in terms of the unit introduced in equation (2).

Dr. See obtains for $\sigma = 0$, $x = 3{\cdot}653962$. To obtain this value he found it necessary to develop his solution as far as x^{50}, an enormous piece of work.

Lane found $3{\cdot}656$ for a different value of k.

The close agreement between these values suggests the question, Is the value really constant for all values of k? If such a theorem could be proved, probably much additional simplicity and elegance could be introduced into the solution of the whole problem.

Dr. See finds from his complete formulæ, of which only a few terms are given above, that the density at the centre of the Sun is six times the mean density. Lane's value of k led to a density at the centre 23 times the mean.

Of course these results depend upon the assumption that the law

$$p \propto \sigma^k$$

holds throughout the interior of the Sun. The laws of gases under pressures and temperatures unattainable in laboratories must clearly be hypothetical, but it is, nevertheless, of great interest to work out numerically a definite hypothesis. We understand, however, that even for temperatures and pressures attainable in laboratories a more correct formula appears to be

$$p \propto \left(\frac{1}{\sigma} - \frac{1}{\Sigma}\right)^{-k},$$

where Σ is the maximum density to which the matter can be squeezed under infinite pressure. This becomes Dr. See's law if Σ be infinite.

We have been puzzled by the fifth section of Dr. See's paper. To us it appears that his hypotheses lead to

$$\text{pressure} \propto (\text{density})^{\frac{5}{3}}.$$

He has, however, calculated from the formula

$$\text{pressure} \propto (\text{density})^{2}.$$

The temperature varies as $(\text{density})^{\frac{2}{3}}$; the constant, however, has to be supplied from hypotheses as to the temperature and pressure at the visible surface. Dr. See considers the truth lies between two hypotheses, according to one of which the temperatures are nine times as great as according to the other. With temperatures of tens of millions of degrees a factor nine is of little consequence.

From his assumptions and subsequent formulæ Dr. See is enabled to calculate the total heat, the potential energy, and the moment of inertia and other problems. He also works out the case for several planets.

In our view, however, his most interesting result is that which he obtains first—that upon certain hypotheses the central density of the Sun is six times the mean.

Addendum.—As Lane used the value $k = 1\cdot4$ and Dr. See uses the value $k = 1\cdot67$, it may be interesting to add the solution for $k = 2$. This particular case corresponds to no physical hypothesis, but it is chosen merely because it happens to be integrable in known functions. For $k = 2$, the boundary condition is that, with Dr. See's notation, the radius of the Sun must be π units of length. As this differs from the value $3\cdot65$ found by Dr. See and

Lane, it shows that the value is not constant for different values of k, as suggested above.

The rest of the solution is

$$\frac{\sigma}{\sigma_0} = \frac{\sin x}{x} = \frac{\sin\frac{\pi r}{\text{R}}}{\frac{\pi r}{\text{R}}}$$

and

$$\mu = \sin x - x\cos x = \sin\frac{\pi r}{\text{R}} - \frac{\pi r}{\text{R}}\cos\frac{\pi r}{\text{R}},$$

where R is the radius of the Sun.

The central density works out to $\frac{\pi^2}{3} = 3{\cdot}29$ times the mean density for $k = 2$ as compared with 23 for $k = 1{\cdot}4$ and 6·0 for $k = 1{\cdot}67$. In this case the temperature varies as the density, and the pressure as the square of either. P. H. C.

[Dr. See has followed this paper by another, on the rigidity of the heavenly bodies, in a later number of the *Ast. Nach.* (No. 4014), of which we hope to give some account in a subsequent number. —Eds.]

NOTES.

Comet Notes.—A comet was discovered photographically by Prof. Kopff at Heidelberg on August 23, magnitude about 11.

The following elliptical elements and ephemeris (for Berlin midnight) of this comet 1906 *e* are by M. Ebell:—

Epoch ……	1906 Sept. 12·5 Berlin.	
M ……….	19° 43′ 29″	
ω ……….	19 33 44	1906·0.
☊ ……….	263 48 45	
i ……….	8 42 8	
φ ……….	31 18 13	
μ ……….	536″·20	
log a …….	0·54712	
Period ……	6·617 years.	
T ……….	1906 May 3·09.	

	R.A. h m s	N. Dec. ° ′
Oct. 4 ….	22 26 20	6 17
8 ….	22 26 15	5 52
12 ….	22 26 36	5 28
16 ….	22 27 22	5 8
Oct. 20 ….	22 28 34	4 49
24 ….	22 30 8	4 33
28 ….	22 32 3	4 20
Nov. 1 ….	22 34 18	4 9

Brightness: Oct. 6, half that at discovery; Nov. 3, one quarter.

Ephemeris for Finlay's Comet (1906 *d*), by L. Schulhof
(Paris Midnight).

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Oct. 4....	7 36 47	20 33	Nov. 9....	8 26 53	21 41
10....	7 50 9	20 42	15....	8 27 52	22 7
16....	8 1 33	20 49	21....	8 26 33	22 40
22....	8 10 57	20 56	27....	8 22 57	23 19
28....	8 18 20	21 7	Dec. 3....	8 17 11	24 2
Nov. 3....	8 23 41	21 21	9....	8 9 25	24 47

Holmes' Comet has been photographically discovered at Heidelberg, being of magnitude 15, and very close to its predicted place.

A. C. D. C.

JUPITER'S SIXTH SATELLITE.—As Jupiter is now approaching Opposition (December 28), search for the small satellites was begun at Greenwich towards the end of August, but owing to unfavourable skies no photographs were obtained until the 28th. On this date, and on August 31, the Sixth Satellite was photographed with the 30-inch reflector, with exposures of 28 minutes and 45 minutes respectively. The results of measures of these plates have been communicated to the Supplementary number of the 'Monthly Notices.' Several similar photographs have been obtained since. It appears that Prof. Perrine was rather more fortunate in his weather, for, from a post-card received from Kiel on September 1, it was learned that the satellite had been photographed at Lick on August 26.

THE VARIATION OF LATITUDE.—In continuation of similar results of previous years*, Prof. Albrecht has published, in *Ast. Nach.* 4121, the result of the observations made at the six latitude stations in the year 1905, which in the main may be summed up by saying that the pole of the Earth is continuing its rapid course about a mean position and is coming inward, the radius-vector at the end of 1905 being about $0''\cdot12$. With these results are published for comparison those from similar observations made at Tokyo, Pulkova, Leiden, and Philadelphia. These are exhibited by setting down in a table for each place the latitude as determined month by month, and applying the variation computed by the formula deduced from the observations made at the latitude stations. A series of determinations of mean latitude is thus arrived at which should naturally be constant. The discordances are on the whole small, although there are some cases where the discordance is as much as a tenth of a second, which is a comparatively large quantity, since the computed variation of latitude is seldom more than 0″·20, but looking at the results one cannot fail to see that they exhibit the fact under investigation.

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* See August number, p. 319.

THE DIRECTION OF GRAVITY IN INDIA.—In last month's number reference was made to a memoir by Col. Burrard on gravity experiments in India, and a result relative to the intensity of this force quoted. Another section of the memoir deals with the direction of gravity, or the deflection of the plumb-line from the normal, and is no less striking.

The method of determining this deflection is briefly as follows:—A certain place is adopted as the origin of the Survey (this origin, selected by Col. Everest many years ago, is Kalianpur, in Central India), the latitude of this place is determined astronomically, and the value is considered final, in fact the astronomical zenith at this place is considered normal to the mean surface of the country. The triangulation being made and lengths measured in miles and yards, the network is superposed on the spheroid adopted as representing the shape and size of the Earth, and the differences of latitude and longitude between the origin and various stations computed. These differences being applied to the latitude and longitude of the origin, give the geodetic co-ordinates of the various stations. Now if the latitude or longitude of these stations is determined astronomically—that is, if the direction of their zenith is determined—they may or may not agree with the geodetic results. If they do not agree, the discordances might be due to the assumed spheroid not being in accordance with facts, or they might be due to non-agreement of the actual zeniths with the normal to the assumed spheroid, that is, to deflections of the plumb-line. It is not difficult to understand how it is possible to discriminate between these two causes of discordance. Now it was expected that there would be found a southerly deflection of gravity through a zone enclosing the main valley of the Ganges and running parallel to the Himalayas for 1000 miles, but that both north and south of this zone northerly deflections would be met with, and to test this prediction Col. Burrard caused observations for latitude to be made at many places along different meridians, with result much as was expected. In the mountainous part of the Sub-Himalayan region the plumb-line was deflected by as much as 30″ northward, in the plains by about 5″, and again south of the zone of southerly deflections above defined the mean displacement was 3″ or 4″ northward. From discussion of the results Col. Burrard finds corrections to the adopted data. In the first place it appears that the best spheroid to represent the facts is one whose major axis is that found by Col. Clarke (which is larger than Bessel's), but with the ellipticity found by Bessel (1 ÷ 299·15), and also it seems that it should not be assumed that the zenith at Kalianpur is normal to the spheroid, but that there is there a deflection of 6″ southward.

NAKED-EYE OBSERVATION OF AN OCCULTATION.—Mr. C. B. Hill writes, in *Popular Astronomy*, that when on a train in California on May 2 last he noticed that Regulus was close to the Moon and that its occultation was imminent, and that he was able to keep the star in sight with his unaided eye up to the moment of disappearance, which he observed. Such an observation is very unusual. The Moon was about nine days old.

OBSERVATIONS OF PHŒBE.—Prof. Barnard publishes observations of the 9th satellite of Saturn made with the 40-inch telescope of the Yerkes Observatory on July 24 and 29. Their reduction shows that the R.A. of the satellite is about $0^s{\cdot}9$ less, and the declination about $3''$ greater, than that of Dr. Ross's ephemeris given in some copies of the American N.A. for 1906. The satellite was also seen on photographs taken at Arequipa in May and June.

INSTRUMENT CATALOGUES.—Messrs. Carl Zeiss have sent us a copy of their Catalogue (Astro 8) of Astronomical Telescopes and Accessories. This is of itself, by reason of its printing and illustrations, quite a work of art, and a study of the pictures is quite a lesson in modern telescope construction. But the main object of such catalogues is, without doubt, to tell possible customers the prices of goods made by the firm, and we therefore here mention some typical items. The cheapest telescope in the list is one on an azimuthal table stand with object-glass of 2⅜ inches aperture, which costs £18 5*s*. A 5⅛-inch refractor on equatorial stand, with accessories, is priced at £162 10*s*., but if the object-glass be a triple lens apochromatic the price is £196. A 7⅞-inch object-glass of this superior kind costs £200.

The Cambridge Scientific Instrument Co. have also sent us an instructive book, which—it may be judged from the title "Technical Thermometry"—has little to do with astronomy, but may be recommended to engineers and manufacturers whose work involves the use of heat as well as to persons engaged in scientific research.

ERRATUM.—On p. 363, line 15 of Mr. Lynn's letter, *for* "4th" *read* "14th." (See also p. 387.)

FOR the benefit of readers in New Zealand, of whom we have a few, attention is called to the rare phenomenon of an occultation of a star by a planet. On the morning of December 10 next, Venus, as seen from places at our antipodes, will occult β Scorpii, but the circumstances are not very favourable. From a note by Dr. Downing in the B. A. A. Journal, published July 26 last, it is learned that, at Wellington, Immersion will take place at Dec. $9^d\ 16^h\ 57^m$ local Standard Time, Emersion $9^d\ 17^h\ 42^m$. The Sun will have risen at

$16^h\ 16^m$. On the east coast of Australia it may be possible to see the emersion, but the immersion will have taken place before Venus rises.

The name of Miss Mary E. Byrd may not be familiar to all our readers, although some years ago there was published in this magazine a breezy article by her entitled "Popular Fallacies about Observatories," and we have had occasion more recently to make commendatory notice of her publications. Miss Byrd has been for 19 years Director of the Smith College Observatory, Northampton, Mass., and we believe has fulfilled her duties as a class-teacher with great success, but now she has voluntarily given up her post and severed her connection with Smith College because of the acceptance by the authorities of money from Mr. Rockefeller and Mr. Carnegie. It is to be gathered from a note in *Popular Astronomy* that this sacrifice—and it is distinctly a sacrifice—has been made purely from devotion to her hereditary Puritan principles, which are not consistent with the belief that such fortunes are amassed by means that would meet with universal approval.

There is a would-be astronomer, Dr. Georg Waltenath of Hamburg, who a few years ago managed to impose on some of the newspapers, so that they made sensational announcements about a "Second Moon of the Earth"—a dark body which was to transit the Sun on a certain day. This scientific person is again putting himself in evidence by giving the Earth—so his circulars say—two dark Moons, which will cause two solar eclipses in the first part of October. One eclipse—partial in Germany and total in France and Italy—is to happen on October 6. The other eclipse will happen about October 3, but the date is not yet exactly fixed. It is well to be prepared for anything.

A note in the *Ast. Nach.* asks that letters for Sir David Gill should now be addressed to him at the Athenæum Club, Pall Mall. It is no secret that this implies his immediate retirement.

The Annual General Meeting of the British Astronomical Association will be on Wednesday, October 31, when the result of the election of Officers will be announced. Mr. F. W. Levander is proposed as President.

From an Oxford Note-Book.

The sad news of the death of Mrs. Pickering on August 29 will evoke deep and widespread sympathy for the Director of the Harvard College Observatory. To many astronomers the loss is that of a dear personal friend, who was indefatigable in welcoming and caring for the visitors attracted to Harvard from all lands by the fame of the Observatory; and who was herself a welcome guest in Europe on more than one occasion, notably at the meeting of the International Astrographic Committee in Paris in 1900. A paragraph in the *Boston Herald* for August 30 states that Mrs. Pickering had been ill for some time. It further reminds us that she was the daughter of Jared Sparks, the famous historian, who was the first McLean Professor of History in Harvard, and was President of the College from 1849 to 1853. She was born in Cambridge on May 1, 1849, and married Prof. Pickering on March 9, 1874.

THE Astrographic Catalogue and the Eros Campaign have introduced us to heavy pieces of labour in measuring star-places, extending over several (sometimes many) years. To lighten and diversify these long journeys is not altogether easy, and devices which might at first sight seem artificial or trivial are not really to be despised. The mere marking of stages is an interest: at Oxford, for instance, we prepared a large diagram with a blank for every plate to be dealt with, and coloured the blank when the plate was successfully taken, adding an emphatic cross when it was measured and reduced. The gradual filling up of this diagram certainly had some influence as a stimulus, though we need not overestimate it. The promise of a day's extra holiday when certain stages were reached was found to produce a distinct acceleration in the measuring; and no doubt other allurements could be devised which would have similar effects. But I have recently heard of an inducement towards rapidity which is quite new to me, though, when it is once mentioned, its simplicity and effectiveness are obvious. A piece of measuring work has been entrusted to some young ladies, and one of them has become engaged to be married. She has (either voluntarily or under torture) given a promise that she will not get married until the work is finished—and all records are being broken.

AT Oxford we are now reading proofs, which may become even more monotonous than measuring. One of the difficulties is to make sure of the correctness of long series of numbers like reference numbers, which change very rarely. The digits representing hundreds, thousands, and tens of thousands are the same for long sequences: and it is difficult to force the eye to read deliberately down the column; it insists on glancing down, and experience shows that a misprint is occasionally passed. Accident suggested recently an expedient for getting more satisfactory results, which is simply to turn the page horizontal and run the eye from left to right. The figures are not in their usual erect position, but it appears to be much easier to detect at a glance any breach of continuity. I imagine this is due to the practice our eyes have in glancing from left to right in ordinary reading. A Japanese or a Chinese might find it better to read the columns in the ordinary way. While writing these words it occurred to me to try an actual experiment for record. I took Vol. I. of the 'Greenwich Astrographic Catalogue' from the shelves, opened it at random at p. 368, noted the time by my watch, and commenced to read horizontally the hundreds and thousands (only) of the reference numbers. In 45 seconds I found an error, viz. on p. 372 the reference number 3629 is printed 3929; in 45 seconds more, with curious exactness, I found another on p. 375, viz. 3959 is printed 3659. But this alarming rate was fortunately not maintained. Proceeding with the intention of stopping at the next mistake I read on for another 20 minutes and reached the end of the zone on p. 432 without finding any other error. It must be confessed, however, that in this time the eye got somewhat tired; and it seems advisable not to continue too long at the operation. In reading actual proofs, however, one would never have more than 8 pages at a time. Of course neither of the above errors matters in the least, for the misprint can be inferred from the context; but there are cases when similar errors may lead to misunderstanding.

MR. FRANK T. BULLEN, author of 'The Cruise of the *Cachalot*' and many other thrilling sea-stories, contributed to the *Dublin Evening Herald* on September 15 an article headed "The Strangest Thing I ever saw." He made the selection "after an afternoon's meditative review of the occurrences of a lifetime," and decides on a meteor-shower which he saw in mid-Atlantic in February 1870, when he was thirteen years old. Certainly, as he describes it, the shower was phenomenal. It opened with a "report as of a great gun," and a "globe of fire so brilliant that its light struck the eye with acute pain," which "rushed around an arc of the heavens leaving a wide band of light behind it, with a hissing as it sped through the wet mist," throwing off vari-coloured sparks or stars. It "almost completed the circuit of the horizon," and finished up with one "perfectly blinding blaze of glory, a definite report, and a smell as of burning sulphur." This was the prelude to a shower of meteors which began quietly and had something of a radiant, but "as they increased in number it was impossible to refer their origin to any given point, or to say that they pursued any general course. The whole heavens from horizon to zenith were literally full of these blazing messengers darting upwards, downwards, athwart, so that the air seemed thick with them." The whole show lasted half an hour and terrified not only the boy of thirteen but the skipper and all the crew nearly out of their wits. As a piece of vivid description the article could scarcely be beaten; but what are we to think of the writer's capacity for remembering actual events? Mr. W. F. Denning, who kindly drew my attention to the article, is clearly sceptical as to some of the details, and our faith in the records of some other stories of wonders related by Mr. Bullen will scarcely be able to stand the strain he has put upon it by describing things of which we think we know something.

In commenting upon the events at the British Association, I forgot to mention an interesting paper by Prof. Larmor and Major Hills on the movements of the Earth's pole. The gist of the paper is that, when allowance is made for the fourteen-monthly "free" oscillation discovered by Chandler, there should remain a forced oscillation in a year, but that the observed figures do not bear this out. We are, I believe, to have an opportunity of hearing the paper in greater detail at an early meeting of the Royal Astronomical Society, and shall then learn more about it. My attention was drawn to a heading, "The Poles and the Revolution," in the *Times* of August 11, under which I expected to find a popular account of the paper, suited to my intelligence; but the article proved to be only about political matters.

Ben Jonson's 'Masque of Queens' was presented at Whitehall February 2, 1609, before the invention of the telescope; but this scarcely excuses the view taken in the following lines of the way in which the evening star first appears. The Second Witch says:—

> I have been gathering wolves haires
> The madd dogges foames and adders eares;
> The spurging of a deadmans eyes
> And all since the evening starre did rise.

An advertisement of the *Popular Science Monthly* announces an article on "Aerography" by Percival Lowell. This *may* be a correct heading for an account of methods of avoiding meteorological disturbances, but is more probably a misprint.

'Thuka of the Moon,' by Charles Hannan (London: Digby, Long, 1906), is scarcely worth the six shillings purchase-money. To begin with, the story is an unpleasing travesty of the fall and redemption of man, the scene being laid in the Moon, where the spirit of evil exists in the form of a Craton—

> a thing of vast size with a hideous shell upon its back and many feet armed with pointed claws, and its face with long neck was most horrible to look upon resembling the features of man stirred by the most evil depths of the depravity of the soul. (p. 14.)

Secondly, as may be gathered from the above extract, the author seems to dislike stops, and his method of printing gives a curious, breathless look to the narrative; sometimes the sense of a passage has hard work to struggle against this incubus:—

> Life was apparently a weak flame placed in the mortal for its appointed time and when that time was over the flame died . . . . there were no hurricanes in the world of the moon nothing but peace . . . . it was not death it was moonlife. (p. 5.)

Nevertheless, the faults of narrative and style might be forgiven the author if he had provided a sufficient number of interesting or amusing extracts for quotation in these Notes. But he has been singularly vague in his descriptions, and I can find nothing better than the following:—

> In the Moon darkness never came, night was unknown to men, and shadows fell only where the mountains raised on high their peaks. Earth was lighted by the reflex light of heaven, for heaven was a land of brilliant silvery light and this light was cast up to the sky through the distance and down again upon the Earth below. (p. 86.)

On comparing this with the following passage, one scarcely feels that the author has been successful in avoiding inconsistency:—

> In this wild flight Ala was coming nearer and nearer unwittingly to the waste of the Moon. Upon this waste the gods looked not, they had placed upon it darkness so that it might not be seen by them as they sat in heaven upon their silver thrones and this darkness lay surrounding the Earth in a density that crushed together and which seemed to shrink back from the purity of the light which filled the sky. There was no night in the world, nor in the waste of the Moon, this thing was darker than night, more terrible and more dense. . . . . The soul grasped at shadows in its presence, all because unreal before this vast blackness where nothing seemed to be! (pp. 175–7.)

After this there is something of an anti-climax in the conclusion of the chapter:—

> This woman seemed to be driven back by the very force of circumstances to that evil which her soul abhorred. In the Moon's waste she had looked upon something very like despair. (p. 177.)

Perhaps a word might be said in favour of the map of the Moon and heaven, which forms the frontispiece, but the exactly appropriate word is not easy to find. Perhaps we may borrow two of the commendations of Mark Twain's map of Paris. The President of the United States remarked: "It is the only map of the kind I ever saw"; and the Emperor of Germany: "I said to my son Frederick William, 'If you could only make a map like that I should be perfectly willing to see you die—even anxious.'"

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A MONTHLY REVIEW OF ASTRONOMY.

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Sun-spots and Magnetism—A Retrospect.

HAVING in former years given my impressions on various matters astronomical with which I had become acquainted in the course of my work at the Royal Observatory, it has occurred to me, from the increased attention of late paid to the question of sun-spots, their periods, and relation with terrestrial magnetism, that this is also a subject on which it may be of interest to recall, so far as I am able, some particulars of its history.

I may premise that the Greenwich Magnetical and Meteorological Observatory, dating from the latter part of the year 1840, was one of a group of such establishments set up in various of our Colonies for the prosecution, for a period of three years, of continuous magnetical and meteorological work. After three years it was resolved to continue the observatories for a further similar period, and ultimately that at Greenwich was made permanent.

Much was looked for from these Observatories and interesting results were before long obtained. On September 25, 1841, there occurred at Greenwich a considerable magnetic storm, an account of which was at once printed by the Astronomer Royal and extensively circulated. Information in due time received (this was not the day of ocean telegraphs) from the Observatories at Toronto, St. Helena, the Cape of Good Hope, and Trevandrum in India, showed that at these places also there had been a magnetic storm, that at Toronto appearing "to have commenced nearly at the same absolute time as at Greenwich and to have been generally simultaneous at both stations." This simultaneity at places so widely separated created much interest, and the preliminary results when received were printed by General Sabine for immediate distribution to the directors of observatories in all parts of the world—an earnest of other important results that might be expected. The remarkable simultaneity in the commencement of magnetic disturbance over the whole Earth, in cases of great disturbance, was afterwards further investigated by me in a paper to be found in vol. 51 of the 'Proceedings of the Royal Society.'

In course of time the observations made at the various Colonial observatories, by officers of the Royal Artillery, came into the hands of General Sabine, who superintended the discussion thereof, communicating the results obtained from time to time in papers to the Royal Society. That "On Periodical Laws discoverable in he mean effects of the larger Magnetic Disturbances, No. II.," read before the Society on May 6, 1852, becomes of interest in the present day from the circumstance that at this early stage, and from comparatively insufficient data, Sabine could so certainly suggest unexpected concurrences between magnetic and solar phenomena that, in later times, and with a far greater wealth of material, have been abundantly confirmed. Indeed our knowledge in regard to the main facts has since scarcely been greatly enlarged, the progress consisting rather in the confirmation and development of the character and nature of the remarkable relation then brought to light. In the paper mentioned we find, Tables 13 to 17, that during the period 1843 to 1848, at two of the Colonial Observatories at which the observations had become discussed, Toronto and Hobarton (stations one in the northern and one in the southern hemisphere), there was found to be continuous increase in the frequency of magnetic disturbance, and also continuous increase in magnitude of the diurnal range of magnetic declination, and further that the increase in diurnal range was just as pronounced in the smaller winter values as in the larger summer values, which, as the winter values of one hemisphere correspond in time to the summer values of the opposite hemisphere, was suggestive of cosmical action. With this impression Sabine wrote: "In our present ignorance of the physical agency by which the periodical magnetic variations are produced, the possibility of the discovery of some cosmical connection which may throw light on a subject yet so obscure, should not be altogether overlooked"; and remarking that Schwabe's independent observation of solar spots showed, between 1843 and 1848, variations similar in character to the magnetic variations, he goes on to say: "As the physical agency by which the phenomena are produced is in both cases unknown to us, our only resource for distinguishing between accidental coincidence and causal connection seems to be perseverance in observation until either the inferences from a possibly too limited deduction are disproved, or until a more extensive induction has sufficed to establish the existence of a connection, although its precise nature may still be imperfectly understood," adding that for such continued observation we must look to those observatories that are permanent, "to those especially which combine astronomical and magnetical research," the Colonial observatories having ceased to exist on the accomplishment of the special objects for which they were established. In a postscript to the same paper Sabine gives, in Table 26, an extended series of values of diurnal range of declination for Toronto and Hobarton, including the years 1841 to 1851, adding thereto values for Munich. These numbers show a minimum of diurnal range about 1843, and a maximum in 1848, in close agreement with the corresponding sun-spot minimum and maximum of 1843 and 1848. The results obtained for other of the Colonial observatories confirmed, apparently in a lesser degree, those found for Toronto and Hobarton.

Sabine was not alone in pointing out the relation of terrestrial magnetism to sun-spots. Dr. Wolf of Zürich and Alfred Gautier both shortly afterwards, independently of each other, found a similar relation with variations of declination range as observed by Lamont at Munich. On which Schwabe enthusiastically wrote, on August 30, 1852, in a letter to Wolf: "Es ist gewiss sehr merkwürdig wie Ihre, Sabine's, Gautier's und Lamont's Beobachtungen übereinstimmen, was bald einigen Aufschluss über die räthselhafte Natur der Sonne geben wird." The question was now for future confirmation or otherwise, but still so far suggestive as distinctly to merit further attention. Dr. Wolf continued to make observations of sun-spots during the remainder of his life, publishing the results in his well-known 'Mittheilungen,' and the general table of sun-spot values which he eventually formed, founded on ancient and modern observations, including his own, acquires its value from the long period of time through which it extends. Prof. Wolfer, his successor at Zürich, continued the work on the same plan, rendering Dr. Wolf's work and his own throughout comparative, the general table having been also revised as other older observations came to light. It was interesting to me to see, when at Zürich some ten years ago, the telescope with which Dr. Wolf made his observations.

If the presumed relation did not at first receive that attention generally that might have been expected, it is to be remembered that the concurrent progression for a few years only of two distinct phenomena, so different in character, might reasonably have been thought to be fortuitous. A curious example of chance agreement, during sixteen years, between the variations of sun-spots and a certain meteorological element with which there could be no distinct relation, is pictorially illustrated in the 'Observatory' for the month of June 1889. On the other hand, the significance of the sun-spot variation, as indicating the existence of far-reaching cosmical action, was scarcely apprehended. The degree of attention given to sun-spot observation in those days, as compared with the rigorous scrutiny which the Sun in our time daily undergoes in all parts of the world, and at all times of total eclipse, in a sense measures the fuller knowledge of the constitution of the Sun that through the agency of photography and the spectroscope has been revealed to us.

We turn now to two papers by Sir George Airy read before the

Royal Society—one "On the Diurnal Inequalities of Terrestrial Magnetism," on April 23, 1863, the other, continuing the subject, on December 10, 1868, in which will be found vector diagrams (combination of diurnal inequalities of magnetic declination and horizontal force in the horizontal plane) for each of the years 1841 to 1863. These diagrams show marked increase in magnitude from 1841 to 1848, very considerable decrease from 1848 to 1857, and again increase from 1857 to 1860, which Airy refers in a general way to changes in the magnetic energy of the Sun (pages 315 and 417 of the two papers respectively), apparently overlooking the circumstance that the vectors, representing the diurnal ranges of magnetic declination and horizontal force, show variations in magnitude in close correspondence with those of sun-spots, the vector maximum of 1848 and minimum of 1857 corresponding to the sun-spot maximum of 1848 and minimum of 1856 quite confirming what Sabine suggested. The question was not, however, lost sight of, for a table given in the first paper is extended in the second paper to include the days of unusual magnetic disturbance at Greenwich in each year from 1841 to 1863, on which it is remarked (page 313 of first paper): "These numbers, as I believe, give a very fair measure of great magnetic disturbance in each year. There is no appearance of decennial cycle in their recurrence"; and at page 414 of second paper: "If they point to any cycle at all, it is one of 6 or 6½ years." In the first paper, page 313, it is further said: "Nor does the number of disturbed days appear to have any distinct relation to the magnitude of diurnal range." The conclusion that magnetic disturbance did not show decennial cycle is not surprising, it is quite a fair inference from the numbers given. For although magnetic disturbance becomes grouped about epochs of sun-spot maxima, it is subject to irregularities (see my chart of magnetic disturbance 1848 to 1897 in the 'Monthly Notices of the Royal Astronomical Society' for December 1899), showing at times in the approach to sun-spot minimum renewed activity, as was the case previous to the minimum of 1856, a period included in that dealt with by Airy, so giving the appearance of a 6-year cycle having, as he remarked, no distinct relation with diurnal range. Had he proceeded one step further and made direct comparison of the variation in magnitude of his vector diagrams with the variation of sun-spots, the accordance between the phenomena, as above pointed out, might have been at once perceived. The observations of sun-spots of Schwabe and Wolf would have provided a continuous record, which could have been employed, although a certain amount of work might have been necessary to put the observations into shape for making the requisite comparison. But there was the discordant result already found from the consideration of frequency of disturbance, offering little inducement to pursue the subject further. It was not at that time known that whilst variation of diurnal range is closely synchronous with variation of sun-spots, which a comparatively short interval of time will show, magnetic disturbance is erratic, as above mentioned, requiring a long period to bring out completely its character, one feature of which is that disturbance in our latitude always dies down at the actual epochs of sun-spot minimum. In the absence of this information it thus happened that the contradictory conclusion arrived at from consideration of disturbance was apparently taken as valid, in which case the character of the result that might have been deduced from consideration of diurnal range would appear comparatively unimportant. And so, by a kind of accident, the Greenwich magnetic results were thought not to confirm relation with sun-spots.

So matters for a time remained. When in the year 1875 I was transferred at the Royal Observatory from the Astronomical to the Magnetical and Meteorological Department, I had not had occasion to consider the question. But in the daily examination of the photographic registers it became in course of time apparent to me that magnetic disturbance was visibly becoming less frequent, in striking contrast to the prolific disturbance of the immediately preceding years 1869 to 1872, and also that the amplitude of diurnal range was similarly diminishing in magnitude, the contemporaneous observation of sun-spots, recently commenced at Greenwich, showing corresponding diminution of solar spot frequency. It was at about this time that Dr. Wolf communicated to the Royal Astronomical Society the paper that appears in Vol. 43 of the 'Memoirs,' containing his table of monthly sun-spot numbers from 1749 to 1876, so giving a favourable opportunity to test whether the Greenwich series of magnetic observations from 1841 onwards would really confirm the relation between magnetism and sun-spots pointed out by Sabine, and remarked in recent years to exist. The result altogether exceeded expectation, showing, both for declination and horizontal force, the variation in magnitude of diurnal range and that of frequency of sun-spots to be in close accord throughout. My paper giving the data employed and the discussion thereof for the period 1841 to 1877 was communicated by Sir George Airy to the Royal Society in 1879, and appears in Vol. 171 of the Phil. Trans. In a further paper contributed to Vol. 63 of the 'Proceedings of the Royal Society,' the inquiry was extended to include the period 1841 to 1896, in which it was further shown that, as found from quiet days alone (free from magnetic disturbance), there was a like relation between variations of diurnal range and sun-spots. In a third paper, that already spoken of as appearing in the 'Monthly Notices of the Royal Astronomical Society' for December 1899, the relation of magnetic disturbance to sun-spots was further considered.

I should further point out that the Astronomer Royal, Sir William Christie, in a communication to the 'Monthly Notices of the Royal Astronomical Society' for November 1889, gave a chart showing, for the years 1873 to 1888, curves not only of whole spots, but also of the faculæ and umbræ, together with curves of the variations of diurnal range of the magnetic elements of declination and of horizontal and vertical force, which, in a further paper communicated to the 'Monthly Notices' for June 1903, was extended to include the years 1873 to 1902. These charts confirm in the most striking manner the fact of concurrent variation between magnetic and sun-spot phenomena. And the relation is shown to be so close for the limited period in all treated, 1841 to 1902, that it seems not an unfair presumption to conclude that it always holds—that is to say, whatever may be (if we may so call them) the freaks of sun-spots, those of terrestrial magnetism are in harmony therewith. The object having been to show that this relation is a reality, which I believe to be sufficiently proved, the problem now is to discover the nature of the relation, whether direct or otherwise. It therefore gave me great pleasure to place any material I had gathered at the disposal of Mr. Maunder when he desired to make such further inquiry (see his papers in the 'Monthly Notices of the Royal Astronomical Society' for November 1904, and April and May 1905), although I do not propose to enter on that separate question here, except to remark that I had sometimes in the past noticed what seemed to be recurrence of disturbance at intervals of a month or so, but without giving further attention thereto.

Prof. Schuster in his paper in Vol. 206 of the Phil. Trans., "On the Periodicities of Sunspots," finds definite periods underlying the irregular sun-spot periods as observed. But the variations of diurnal range of magnetism at Greenwich so far as examined, 1841 to 1902, entirely synchronise with those of sun-spots, in the length of individual periods, relative intensity, and other points. It would therefore seem, by analogy, that underlying periods found generally to exist in the irregular sun-spot variations as observed should similarly exist in the synchronous irregular magnetic variations as observed.

The subject of the relation of terrestrial magnetism to sun-spots has been pursued by others, not only in this country but also abroad, but of the labours of others it is beyond the scope of this paper to speak. Indeed I am scarcely in a position adequately to describe what in other directions has been done, my narrative being simply some history of such part of the work as attaches to Greenwich. W. Ellis.

The Number of the Visible Stars.

The total number of the stars visible in the largest telescopes and on photographs is usually estimated at about 100 millions. To test the probable accuracy of this estimate I have made a number of counts of stars on the photographic prints given in the late

Dr. Roberts's volumes of stellar photographs. These counts were made by means of a *réseau* ruled in squares on transparent celluloid. By careful measurement I find that the side of a square on the *réseau* used is 7·62 millimetres, and therefore the area of each square is 58 square millimetres. By counting the number of stars in a square in various parts of the print a good average may be obtained, and as the scale of the photograph is always given, it is easy to compute the probable number of stars per square degree on each plate.

The photographic prints show stars down to the 16th or 17th magnitude, but probably some faint stars visible on the original plates have been lost in the reproductions. The time of exposure of the plates varies from $1^h\ 30^m$ to 4^h; but this seems to be of little consequence, for Dr. Roberts says, in the Preface to the second volume (1899):—

"(A) Eleven years ago photographs of the Great Nebula in *Andromeda* were taken with the 20-inch reflector and exposures of the plates during intervals up to four hours; and upon some of them were depicted stars to the faintness of 17th to 18th magnitude, and nebulosity to an equal degree of faintness. The films of the plates obtainable in those days were less sensitive than those that have been available during the past five years, and during this period photographs of the nebula with exposures up to four hours have been taken with the 20-inch reflector. No extensions of the nebulosity, however, nor increase in the number of the stars, can be seen on the later rapid plates than were depicted upon the earlier slower ones, though the star-images and the nebulosity have greater density on the later plates.

"(B) The Great Nebula in *Orion* has been photographed with the 20-inch reflector at frequent intervals between the years 1886 and 1898 with exposures varying between one minute of time and seven hours thirty-five minutes; yet the stars are not more numerous or the extensions of the nebulosity greater on the latter than are shown on a plate of like sensitiveness which had been exposed during ninety minutes only; the difference exhibited was that of density.

"(C) The group of the *Pleiades* has been photographed with the 20-inch reflector during numerous intervals between 1886 and 1898 with exposures of between one minute and twelve hours. The results are that only the same faint stars and nebulosity seen upon plates which have had an exposure of one and a half hours are depicted upon those which have been exposed during ten or twelve hours.

"(D) Several photographs of the region of the *Milky Way* in *Cygnus* (Plate 5) have been taken with the 20-inch reflector between the years 1886 and 1898, and on comparing two of them (one with an exposure of 60^m and the other with $2^h\ 35^m$) no fainter stars could be found on one than on the other; there is no reason for suspecting that this result was due to some abnormal condition in either case, and it has been confirmed by photographs which have been taken in other areas in the sky."

In making the counts I have carefully avoided the clusters and nebulæ shown on each plate. The following table shows the results of the counts made, and the region of the sky:—

Counts of Stars.

Region.	Approx. R.A.	Approx. Dec.	No. of Counts.	No. of stars per square degree.	Exposure.	Remarks.
	h m	° ′			h m	
Near M. 31	0 37	+40 43	40	1466	1 30	Near great nebula in Andromeda.
„ M. 37	5 46	+32 31	50	1939	1 30	Auriga; near Milky Way.
„ N.G.C. 2237	6 26	+ 5 0	50	1823	2 45	Milky Way; Monoceros.
„ M. 46	7 37	−14 36	25	2656	1 30	„ „
„ H. V. 37 ...	7 55	−10 21	30	1994	1 30	Argo; edge of Milky Way.
„ H. I. 200 ...	8 46	+33 48	600 sq.	481	4 0	Leo Minor; non-Galactic
„ H. I. 56 ...	9 26	+21 57	600 sq.	245	4 0	Leo; non-Galactic.
„ H. I. 168 ...	10 12	+41 55	600 sq.	282	4 0	Ursa Major; non-Galactic.
„ M. 11	18 46	− 6 23	30	3180	1 30	Milky Way; mostly small stars.
„ H. VI. 38...	19 27	+ 9 1	30	4320	2 0	Milky Way; Aquila.
Cygnus	19 45	+35 29	50	5460	2 35	Milky Way; north of η and χ Cygni.
Near M. 71	19 49	+18 31	20	5040	1 30	Milky Way; Sagitta.
„ H. V. 14 ...	20 52	+31 18	15	5000	2 55	Milky Way; Cygnus.
In Cygnus	20 52·6	+47 2	40	4470	2 0	„ „
Near H. VII. 52 .	21 26	+46 39	20	5290	1 30	„ „
„ H. I. 53 ...	22 32	+33 54	50	1730	3 39	Pegasus; south of Milky Way.
„ H. I. 55 ...	23 0	+11 47	600 sq.	435	4 0	Pegasus; non-Galactic.
„ H. II. 240 .	23 58	+15 36	600 sq.	596	4 0	„ „

From the above table it will be seen that the number of stars per square degree ranges from 5460 stars in a rich region of the Milky Way down to 245 in a non-Galactic region. Some portions of the Milky Way, such as those in Auriga and Monoceros, are less rich in stars. These parts are fainter than those in Cygnus, which have a larger number of stars per square degree. This shows clearly that the fainter portions of the Milky Way owe their faintness to a smaller number of stars and not to greater distance from the Earth, as some astronomers have erroneously supposed.

The average results from the above table are as follows:—

Milky Way	4137	stars per square degree.
Near Milky Way ..	1782	„ „
Non-Galactic	408	„ „

Prof. E. C. Pickering gives the following estimates of the areas covered by the Milky Way and the non-Galactic regions:—

Rich Milky Way........	10,999	square degrees.
Fainter portions	4,613	„ „
No Milky Way	25,641	„ „

Hence we have

10,999 × 4137 = 45,502,863
4,613 × 1782 = 8,220,366
25,641 × 408 = 10,461,528

Total 64,184,757

The real total may be somewhat greater, as probably some of the fainter stars visible on his plates are lost in the prints given in Dr. Roberts's volumes.

I have also by the same method counted the number of stars in some of the irregular clusters shown in Dr. Roberts's photographs, with the following results:—

Stars in Irregular Clusters.

Cluster.	R.A.	Decl.	Area in square minutes.	No. of stars in Cluster.	No. of stars per square degree.	Remarks.
	h m	° ′				
Messier 36...	5 29·6	+34 4	223	183	2948	
„ 37...	5 45·6	+32 51	343	580	6052	
„ 35...	6 2·7	+24 21	390	332	3103	A comparatively pour cluster.
„ 50...	6 58	− 8 12	186	130	2520	
„ 46...	7 37	−14 35	319	510	5700	
„ 67...	8 46	+12 11	158	184	4190	
„ 103...	11 26·6	+60 11	37	73	7000	
„ 26...	18 40	− 9 29	56	86	5560	
H. VI. 32 ...	21 27	+51 8	72	126	6200	
H. VII. 66...	21 43	+65 20	116	264	8200	
Messier 52...	23 20	+61 3	121	189	5430	An unusually rich cluster.
H. VI. 30 ...	23 52	+56 10	186	625	12122	

The average richness of these clusters is 5752 stars per square degree, or only a little more than the richest parts of the Milky May. The globular clusters are, of course, richer than this. Prof. and Mrs. Bailey's counts of stars in the great globular cluster ω Centauri gives about 25,000 stars to the square degree. This would give for the whole heavens, if equally rich, over 1000 millions of stars. But as ω Centauri is, of course, *unusually* rich in stars, we may conclude with confidence that the actual number of the *visible* stars falls far short of 1000 millions. An estimate of 100 millions is probably a maximum.

Dublin, 1906, Oct. 5. J. Ellard Gore.

The Planets and Planetary Observation.

[Continued from p. 380.]

From a study of the available materials it seems difficult to assign any rotation-period that shall prove thoroughly satisfactory and accord with a fair amount of the observational evidence. The apparent persistency in the visibility of some of the spots during successive days and their fixed positions, pointedly suggest a long period, while other markings, changing at short intervals, strongly support a much swifter rate. If the features could be discerned with facility and certainty and their exact forms and places often determined, it would soon be possible to get a tangible result out of them; but the number of observations is comparatively small, and some of them are scarcely of a character to inspire confidence. Dealing with miscellaneous materials of this sort, it might be easy to support one theory or particular period by carefully selecting a portion of the records supporting it, and giving them undue weight. But results obtained by such methods are worse than useless, since they are mere delusions put forth under the guise of legitimate deductions.

The rotation-period of the planet, as deduced by several observers, exhibits considerable discordance and we cannot certainly accept any individual result as having been reliably demonstrated. The following table is a summary of the values assigned:—

1.—1800. J. H. Schröter	24^{h}	4^{m}	0^{s}	Rounding of S. horn in March and April 1800.
2.—1801. C. L. Harding	24	5	28	Dusky spot near S. horn.
3.—1810. F. W. Bessel	24	0	50	5 obs. by Schröter during 14 months.
4.—1816. C. L. Harding	24	0	50	Obs. by Harding and Bessell.
5.—1890. G. V. Schiaparelli ...	87^{d}	23^{h}	1^{m}	Observations 1882–1889.
6.—1897. L. Brenner............	33^{h}	45^{m}	0^{s}	22 obs., May to August 1896.
7.—1904. J. McHarg	24	8	0	Dark band obs. April 14–25, 1904.
8.—1904. J. McHarg	24	5	48	Blunting S. horn seen by Schröter (1800 March) and Denning (1882 Nov. 5).
9.—1906. W. F. Denning	24	42	0	Comparison of various spots drawn by different observers.

1.—Schröter questioned the accuracy of the period to within a few minutes.

2.—This is variously given in different books as $24^{h}\,5^{m}\,28^{s}{\cdot}3$, $24^{h}\,5^{m}\,28^{s}{\cdot}13$, $24^{h}\,5^{m}\,30^{s}$, &c.

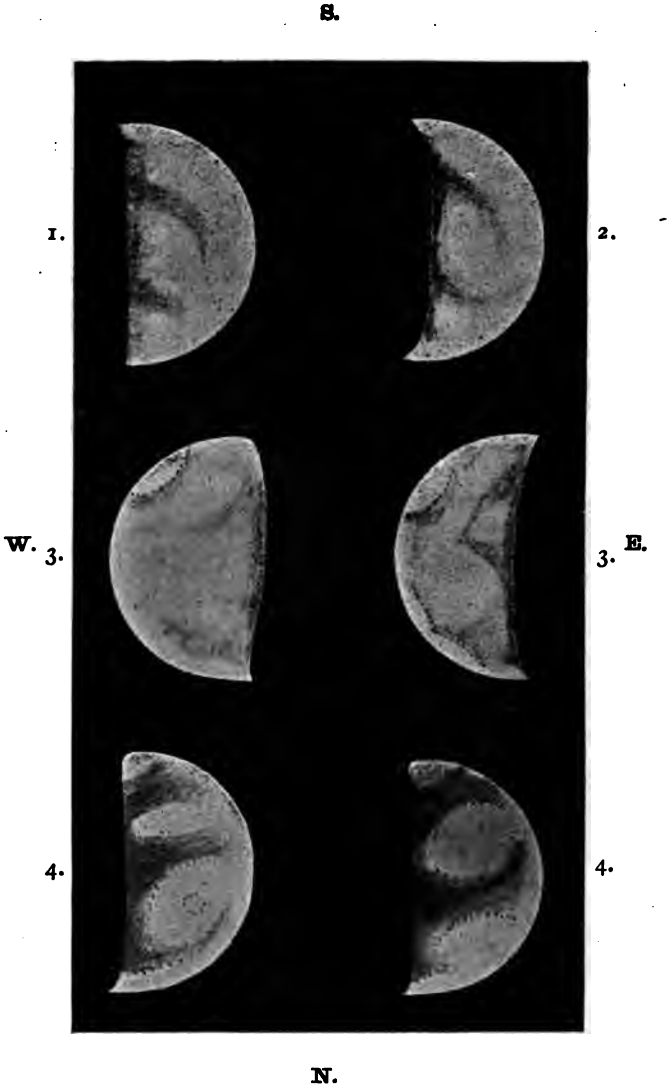
5.—Value arrived at after thorough investigation. Mr. Lowell corroborates this period.

6.—Period between 33 and 35 hours, most probably $33^{h}{\cdot}75$. *B. A. A. Journal*, vii. 392.

8.—Exceedingly doubtful value owing to the lack of intermediate observations.

9.—Somewhat uncertain, but the value is strongly indicated by a portion of the observations.

In his 'Descriptive Astronomy,' 3rd ed. p. 61, Mr. G. F.



	h	m		h	m
Brenner, 1896, July 9	22	0	2. Molesworth, 1898, June 2 ...	13	5
Molesworth, 1898, Apr. 2 ...	21	50	3. Molesworth, 1898, Apr. 8 ...	22	35
Denning, 1882, Nov. 9	19	39	4. Denning, 1906, Sept. 1	18	10

Chambers gives the rotation-period determined by Schröter in 1801 as $24^h\ 5^m\ 48^s$. Sir J. F. Herschel, in his 'Outlines,' 11th ed. p. 730, gives $21^h\ 5^m$, but this is probably a misprint for $24^h\ 5^m$. At Bristol, in 1882 November, my observations indicated a rotation in approximately 25 hours, but the value could not be very exactly assigned. Mr. MacEwen favours Schiaparelli's long period of rotation, for he says that on two occasions in 1892 he watched markings on Mercury for 2½ hours, and was struck with their stationary positions during that time.

The observations already made will afford an excellent means of comparison and ought to be of material assistance to those who further investigate the axial rotation of Mercury. That the true rate of this will soon be ascertained appears probable, and the reason that it has not, perhaps, been already fixed is due to negligence of a promising object, affected, however, by conditions rendering observations difficult to procure.

During transits of Mercury across the Sun some curious features appear to have been recognized, and it may be useful to summarize a portion of the observations:—

1697 Nov. 3.—Wurzelbaur saw a grey spot on the dark planet.

1736 Nov. 11.—De Plantade remarked that the disc of the planet was surrounded by a luminous ring.

1799 May 7.—Schröter and Harding distinguished two spots and a luminous halo about ¼ the diameter of Mercury.

1802 Nov. 9.—Fritsch and others saw a white spot. Lyunberg saw Mercury surrounded with a dark zone, but Herschel on the same day saw its "preceding limb cut the luminous solar clouds with the most perfect sharpness." Humboldt says that at Peru, during this transit, he paid great attention to the sharpness of the outline of the planet during the emersion, but did not remark anything resembling an envelope.

1832 May 5.—Moll noticed a ring encircling the planet, and also detected a spot on the disc. Harding saw two spots, Gruithuisen one.

1868* Nov. 5.—Mr. Huggins saw one spot near the centre of Mercury and a luminous aureola fringing the disc and ⅓ its diameter. Pratt observed the aureola by projecting the Sun's image in a darkened room. Browning saw an aureola slightly brighter than the solar disc, and two bright grey spots close together near the centre of the planet. Knott says the disc was uniformly black, and that no white spot could be seen.

1878 May 6.—Mr. Christie, Mr. Dunkin, and Mr. Criswick remarked an annulus round the planet. Rev. E. Ledger also saw it in Suffolk. The width of the halo was the same as Mr. Criswick estimated, viz. about 2-3rds of the planet's disc. The former also perceived a bright spot a little S.W. of the centre.

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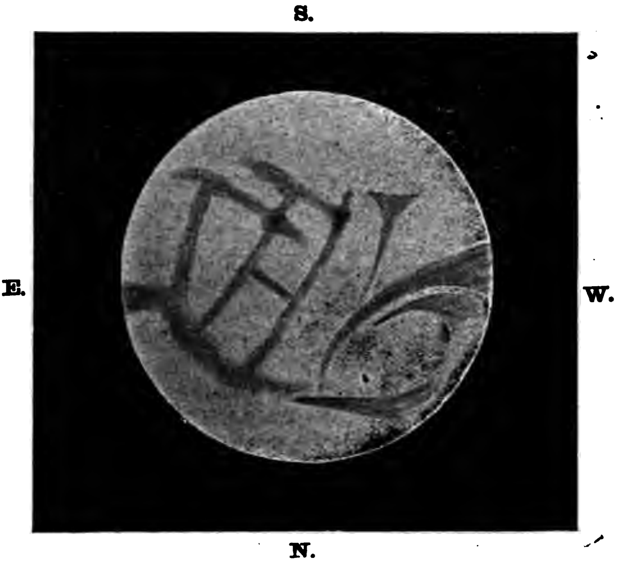
* The writer at Bristol specially looked for the white spot and luminous ring at the transit on November 5 of this year, but they were invisible in a 4¼-inch refractor.

1881 Nov. 8.—Mr. Tebbutt saw a faint whitish spot at the centre, and Dr. Little, at Shanghai, a darkish halo round Mercury, but no included white spot.

1891 May 9.—Prof. Naegamavala noticed the aureola surrounding the disc, but no bright spot (16¼-in. Refl., p. 130). The Rev. A. de Penaranda says the aureola was distinctly seen of a decidedly yellow tinge. At the Observatory of Vizagapatam the observers "did not find a ring of faint light around the planet" (6-in. O.G.). Mr. Macdonnell says no traces of a luminous halo were visible on this occasion (6-in. O.G., p. 110 and 200).

Some of the observers describe the annulus, aureola, or luminous ring as *brighter* than the bordering solar surface, others as *darker*.



Schiaparelli's Map of Markings on Mercury.

The estimates of its diameter vary a good deal. But it is significant that the majority of those who have critically viewed these transits (and notably in 1868) remarked neither the annulus nor the bright spot or spots. We are inclined to summarily dismiss the phenomena alluded to as optical apparitions. Webb thought they were probably deceptions from the violent contrast and fatigue of the eye. Prof. Powell suggested that diffraction of light contributed to their formation, but this is not accepted. Another view is that the white spot has had its origin in the internal reflection of light in the eyepiece. But though the white spot may be explained away, the annulus is not so readily accounted for. Possibly the want of perfect achromatism in object-glasses and eyepieces has been partly responsible for the coloured aureola. But though a considerable number of observers have detected this feature, a greater number have not; the evidence furnishes a contradiction which astronomers will remember while watching future transits, the dates of occurrence of which during the present century will be as under:—

1907 Nov. 12.
1914 Nov. 6.
1924 May 7.
1927 Nov. 8.
1937 May 10.
1940 Nov. 12.
1953 Nov. 13.
1960 Nov. 6.
1970 May 9.
1973 Nov. 9.
1986 Nov. 12.
1999 Nov. 24.

The conditions under which Mercury is presented alternately as a morning and evening star are repeated at nearly the same dates every thirteen years; thus the elongations of 1894 will recur only four days later in 1907.

[To be continued.]

The Sun-spots of 1906.—IV.

In 1906 August there was a marked falling off in the sun-spot activity as compared with the earlier months of the year. July bequeathed to August two great groups, Nos. 5931 and 5933, and both these groups returned again later in the month, as Nos. 5946 and 5944 respectively. Group 5944 also became a part of a widespread disturbance spreading to the north and west. But with the exception of these two groups and their dependents, there was only one noteworthy spot-group observed during the month—Group 5942, a pretty stream of the usual form,—and the great majority of the groups were quite small.

Rotations 707 and 708 began August $1^{d}.23$ and $28^{d}.47$ respectively.

Notes on the Principal Spot-Groups.

Group 5942.—Stream of normal type. Area about 120.

Group 5944.—Return of Group 5933. A large regular spot with some distant companions, forming a very scattered stream, much inclined to the equator. Area about 160.

Group 5946.—Return of Group 5931. Fourth apparition. Somewhat diminished from third apparition, and still slowly declining. Area about 200.

Group 5950.—Fine stream forming north of Group 5944, and forming with it, and with Groups 5951 and 5952, one great area of disturbance. Area about 300.

Spot-Groups in 1906 August.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	d	°	°	
5935	Aug. 7	Aug. 19	13	Aug. 13·3	200	+18	Regular spot.
6	8	17	10	12·2	215	+17	Small spot.
7	9	10	2	9·3	253	+ 2	Very small spot.
8	11	18	8	13·6	197	−19	Short stream.
9	14	17	4	16·7	155	+ 7	One or two small spots.
5940	16	17	2	12·3	214	−18	Small faint spot.
1	18	18	1	16·0	165	−21	Short irregular stream.
2	18	25	8	20·6	104	+10	Well-developed stream.
3	20	20	1	20·4	107	+ 1	Few small spots.
4	20	Sept. 1	13	26·6	25	−18	**Return.** Regular.
5	21	Aug. 25	5	22·0	86	+ 4	Two small spots.
6	22	Sept. 3	13	28·9	355	+22	**Return.** Regular.
7	23	Aug. 24	2	22·4	80	−19	Broken ring.
8	24	25	2	22·0	86	+ 7	Small spot *n*. Gr. 5945.
9	24	24	1	22·7	76	−18	Small spot *f*. Gr. 5947.
5950	24	Sept. 1	9	26·7	24	− 7	Fine stream *n*. Gr. 5944.
1	24	Aug. 28	5	27·6	11	−12	Small stream *sf*. Gr. 5950.
2	26	Sept. 2	8	27·2	17	−11	Small, regular, *p*. Gr. 5951.
3	26	Aug. 29	3	26·8	22	+10	Small. Intermittent.
4	28	Sept. 1	5	30·9	328	−13	Ring-shaped group.
5	29	1	4	Sept. 2·9	288	−10	Double spot.
6	29	7	10	4·0	273	+ 4	Small spot.
7	30	3	4	4·3	269	+14	Very small. Intermittent.

Group 5954.—A chain of small spots forming an elliptical ring. Area about 70.

Group 5956. — Well-marked double spot, breaking up into a compact stream. Area about 80.

E. Walter Maunder.

A Memoir on Double Stars [*].

The Royal Astronomical Society has again shown its appreciation of the importance of double-star investigations by adding Mr. Lewis's great work to its already long list of memoirs on this subject. This publication is of more than passing interest, not only on account of the many investigations it contains, but also for its collections of observations, elements, historical notes, and critical remarks. Its practical importance will be understood from a glance at the history of the stars to which it relates, and from a consideration of the present state of double-star astronomy.

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[*] 'Measures of the Double Stars contained in the Mensuræ Micrometricæ of F. G. W. Struve.' By T. Lewis, Secretary R.A.S. Mem. R.A.S. vol. lvi.

In 1822, Wilhelm Struve published a catalogue of 795 double stars, including practically all that were then known and accessible to northern observers, most of them the discoveries of Sir William Herschel. Soon after this Struve began his extended explorations with the Dorpat refractor, and brought to light so many new pairs that an enlarged catalogue became necessary, which was issued under the title 'Catalogus Novus Stellarum Duplicium et Multiplicium.' This work, popularly known as the "Dorpat Catalogue," contains 3112 entries. But when Struve came to measure the objects enumerated in it, 23 of them could not be found in the sky, and besides it was then ascertained that 27 had been entered in duplicate, reducing the number of objects to 3062. Some hundreds of these were regarded as unworthy of retention, and for various reasons were rejected, leaving 2618, to which 22 were afterwards added in 'Mensuræ Micrometricæ,' bringing the revised total up to 2640. It is these stars that Mr. Lewis has especially considered. But various observers having from time to time measured some of the rejected stars, Mr. Lewis has included certain of these, so that his work has to deal with 2693 Struve pairs.

Perhaps nothing that was done early in double-star astronomy had greater directive effect than the publication by Struve in 1837 of 'Mensuræ Micrometricæ,' a ponderous folio, giving his measurements of the double stars of the Dorpat Catalogue. This work, it may be said, reviewed the whole of double-star astronomy to that epoch. A great variety of topics are considered in its introduction, while measures and notes make up the bulk of the volume. Even at that time some of the pairs had comparatively extensive histories, and many more gave promise, which has since been amply fulfilled, of increasing interest in the future. Attention was called to these, and with Struve's work to serve as a model it was but natural that subsequent observers and investigators should largely follow the course indicated by him. Hence, notwithstanding the numerous discoveries that have since been made, the stars of the Dorpat Catalogue stand well apart from all others, and for historical reasons it seems fitting that they should remain so. Therefore, Mr. Lewis's work perpetuating this list and bringing the accounts of its members to date, thus bridging the nearly three-quarters of a century that has elapsed since the publication of 'Mensuræ Micrometricæ,' is most welcome.

Altogether, more than thirteen thousand objects are included in the various catalogues of double stars, ranging in difficulty from easy pairs readily separable with slight optical assistance to those whose resolution taxes to the utmost the highest powers of the greatest telescopes. These stars vary widely in interest, importance, and the labour that has been bestowed upon them. Of those that have been known the longest, some which were worthy of greater care have at times been much neglected, while others, for no sufficient reason, have been the recipients of nearly continuous attention. Altogether a vast amount of work has been done upon the double stars, but not out of proportion to the magnitude of the subject nor the importance of the results. The thousands of observations afford data for numerous investigations which have greatly increased our knowledge of the stars, especially in respect to their motions, masses, distances, and relationships. But the work has been done by many people in many places, and the printed results are scattered widely in astronomical periodicals, observatory publications, proceedings of learned societies, and miscellaneous papers. With so many objects demanding attention, with the information respecting them unindexed, with the observations and theoretical results accumulating from year to year and being ever more and more widely scattered, it has for some time been increasingly difficult for those interested to arrange programmes of double-star work that shall in the largest measure effectively contribute to progress. Need has been felt in this department of more accessible lists of the known double stars, of condensed and reliable accounts of them, with such collections of the measures as will enable one without loss of time to see what has been done in the case of any given pair. Some relief in these directions has already been obtained by the publication of Mr. Innes's 'Reference Catalogue of Southern Double Stars'; of Prof. Burnham's 'A General Catalogue of 1290 Double Stars,' his own discoveries; and of the writer's 'Micrometrical Observations of the Double Stars Discovered at Pulkowa.' Now Mr. Lewis has further cleared the way by giving us his 'Measures of the Double Stars contained in Mensuræ Micrometricæ.'

Mr. Lewis has presented these stars in the order of their numbers, with right ascensions and north polar distances for the epoch 1900, and magnitudes and colours according to Struve. He has collected the micrometrical measures and given annual means, by observers, in chronological order. The orbits of many of the binaries have been investigated, and in addition to his own results Mr. Lewis has tabulated the elements obtained by other computers. Diagrams accompany many of the investigations, and sometimes the apparent orbit of the principal star, derived from meridian observations, is given in addition to that obtained from the micrometrical measures. To keep the book dealing with so many objects within reasonable limits as to size the notes have been restricted to salient facts. Nevertheless, considerable space has been required for the discussion of historic pairs, which are not only numerous in this list but have been the subjects of frequent investigations.

The statistical study of the double stars is certain to lead to interesting results as soon as the circumstances of the relative motions of the components of a sufficient number of pairs have been ascertained. The majority of double stars have been discovered so recently, however, that nothing is yet known concerning either their absolute or their relative motions, and it will doubtless be many years before these are ascertained for the greater part of them. At present statistical studies depending upon the fixity or non-fixity of components must therefore be sought in older lists, of which that of Struve is the most important. Mr. Lewis in the introduction to his volume gives us the conclusions drawn from some of his investigations in this direction. These are interesting and suggestive and relate particularly to the distances of these systems from us and to the relative masses of their components.

He has first compared the number of Struvian double stars in each hour of right ascension from the north pole to fifteen degrees south declination with the corresponding number of stars in the same region given in the 'Bonn Durchmusterung.' This comparison is given in a diagram, where one curve shows "the distribution in each hour of Right Ascension of 405,000 stars in Argelander's Catalogue from the North Pole to 15° South Declination. A curve below it shows the distribution of the stars in the 'Mensuræ Micrometricæ.' The resemblance between the two curves is most marked, and bearing in mind that it is deduced from 3000 stars only, the double-star curve is not uneven. We may conclude, therefore, that the first result of our inquiry is that the distribution of the double stars follows very closely the distribution of the stars in general." Where the unit area of comparison is so large, approximating one-fortieth of the celestial sphere, and the number of available pairs used so small, the above generalization appears true. My own experience, however, in examining many thousands of stars very carefully with the refractors of the Lick Observatory, for the express purpose of ascertaining which of them are double, has led me to believe that the double stars are relatively more numerous in some parts of the sky than in others, and when the subject comes to be worked up in detail I shall be much surprised if it does not prove that the face of the sky is quite spotted in respect to the distribution of visual pairs *.

Mr. Lewis has next compared the numbers of double stars whose components have not yet exhibited any relative motion with those that do show such motion. The former are on the average 1·81 times as numerous as the latter; but the ratio varies in different hours of right ascension, being constantly above the average from one hour to fourteen hours, and below it from fourteen hours to one hour. In this connection Mr. Lewis says:—

"There is no apparent reason why distant stars should not have

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* [It is to be noted that the curves above described are intended by Mr. Lewis to be considered essentially in connection with the curve showing the comparative numbers of the pairs which have relative motion and those which have not.—H. P. H.]

close companions endowed with orbital motion as frequently as those nearer to us, nor is there any known reason why the double stars should not be evenly distributed in space. If we find this distribution is not uniform, it may be that in certain regions the stars are, as a whole, more distant from us than in others. Granting this, the proportion of fixed pairs to pairs showing the relative motion should be a function of the distance of the stars from us." This is the explanation he offers, and based upon it he derives the relative distances of the double stars in various directions. He then adds: "The points thus brought out seem to show that the stars around us form a universe very much the shape of an egg, and that we are not in the centre. The longer diameter may be roughly estimated as equivalent to 600 light years, and the smaller to 300 light years. Professor Seeliger considers the Milky Way in its nearest parts as distant from us some 4,400 light years. If so, the Milky Way evidently lies beyond the systems here discussed."

Finally, Mr. Lewis has obtained the relative masses of the components of eighteen binaries, all that have sufficient data for the investigations. The results have been obtained by comparing the absolute motion of the principal star, derived from meridian observations, with the relative motion of the companion, deduced from micrometrical measures. The values are most surprising, and the method is doubtless one with a future. One would have expected preponderance of light and mass to go together; but if the values obtained in these researches hold true for double stars in general, the inevitable conclusion will be that usually the fainter star in a binary combination is the more massive and that the brighter component is really the satellite.

Detroit Observatory,
Ann Arbor, Michigan.

W. J. Hussey.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Shape of Saturn.

Gentlemen,—

It is well known that in the year 1805 W. Herschel noted a peculiar appearance in the shape of the planet Saturn, which is usually described as rendering it square-shouldered. Bessel, however, from observations of his own in 1832, contended that this was an illusion and that the disc was strictly elliptical in figure. Main asserted this to be the case even more strongly on the strength of observations made by him at Greenwich in 1848 (when the ring had disappeared), confirmed by others at Oxford in 1862. Proctor, nevertheless, takes the view that their opposition to Herschel's statements is founded on the erroneous notion that the latter contended for a persistently abnormal figure of the planet, whereas what he intended was that it was subject to temporary fluctuations. In 1806 the planet appeared to Herschel less distorted than in 1805; in 1807 the distortions were more irregular, flattened in the northern regions, more curved or bulged outwards in the southern. To confirm his own perception he asked his son to draw the planet as it appeared to him on a slate, when he exactly delineated the appearance described by his father. This was on the 16th of June, 1807, and is interesting as being the first recorded observation of John Herschel, then little more than fifteen years of age. Prof. Wilson, who had then retired from Glasgow and was residing at Hampstead, where he had one of Herschel's 7-feet reflectors, being asked to examine the planet, found the shape on the 23rd of June something like that above described, and called it an illusion; and Herschel himself ascribed the cause to a peculiar refraction produced by the neighbouring portion of the ring. Proctor remarks that this supposed effect is "not one which can be admitted as possible," but ascribes the appearance undoubtedly seen by Herschel to fluctuations in the condition of the atmospheric matter surrounding the planet. These appearances have been noticed by other skilful observers, but only on very rare occasions. Mr. Denning noticed something similar in the autumn of 1880, when observing Saturn with a 10-inch reflector, but considers that the singular figure was "due to the contrasting effects of the belts" ('Monthly Notices,' vol. xli. p. 82, and 'Telescopic Work for Starlight Evenings,' p. 197). The peculiarity was less marked at the end of October and the beginning of November than earlier. The difficulty of explaining the appearance by the belts is that nothing similar has apparently been recorded in the shape of Jupiter. The subject seems to deserve further study; but Proctor was justified in the remark that the observations of Bessel and Main were beside the question, as Herschel never supposed that the shape in question was permanently to be seen.

Yours faithfully,

Blackheath, 1906, Oct. 3. W. T. LYNN.

Dr. See's Researches.

GENTLEMEN,—

In the current number of the *Observatory* a notice of Dr. See's papers in the *Astronomische Nachrichten* (Nos. 4053 and 4104) is given. Dr. See's results are open to criticism in many respects, and I have sent to the *Astronomische Nachrichten* a note which I hope will soon be published, and in which it is more especially pointed out that the calculations of temperature throughout Dr. See's papers are based on a misunderstanding. The temperature of a mass of gas subject to the adiabatic law depends, if the distribution of pressure and density be known, on the nature of the gas only, and there can be no alternatives like those given by Dr. See. He further uses the term rigidity in a sense hitherto unknown to science. His "rigidity" is not a property of the body, but simply "pressure."

At the end of your notice reference is made to the particular value of the ratio of the specific heats ($k = 2$) for which the solution of the problem may be found in finite form.

May I remind those interested in the subject that there is another value of the ratio of specific heats ($k = 1{\cdot}2$) for which the complete solution may be obtained? I published the equations at the first Southport Meeting of the British Association (1883). The matter is, perhaps, of sufficient interest to give the results here, as they seem generally to have been overlooked. If p be the pressure, ρ the density, g the constant of gravitational attraction, r the distance from the centre, M the mass included in a sphere of radius r,

$$p = \frac{27}{A^3(2\pi g)^{\frac{3}{2}}} \cdot \frac{c^3}{(c^2+r^2)^3},$$

$$\rho = \frac{9\sqrt{3}}{A^{\frac{3}{2}}(2\pi g)^{\frac{5}{4}}} \cdot \frac{c^{\frac{5}{2}}}{(c^2+r^2)^{\frac{5}{2}}},$$

$$M = \frac{6\sqrt{3}\sqrt{c}}{A^{\frac{3}{2}}g^{\frac{5}{4}}(2\pi)^{\frac{1}{4}}} \frac{r^3}{(c^2+r^2)^{\frac{3}{2}}}.$$

The total mass is

$$\frac{6\sqrt{3}\sqrt{c}}{A^{\frac{3}{2}}g^{\frac{5}{4}}(2\pi)^{\frac{1}{4}}},$$

which remains constant as the body is contracting. "A" depends on the nature of the gas. It is known, if the pressure and density are known at any one point, through the relation

$$\rho = Ap^{\frac{5}{6}}.$$

As the mass contracts, A changes.

I should like further to mention that the conclusions of the last paragraph but one, and the one preceding it, in the paper as published in the Reports of the British Association were founded on some confusion of thought, but otherwise the paper, as far as I know, is correct.

Yours faithfully,
ARTHUR SCHUSTER.

Victoria Park, Manchester,
1906, Oct. 5.

[It should be said that in his later paper in *Ast. Nach.* No. 4104 * Dr. See makes frequent reference to Prof. Schuster's researche and says in one place: "Some of the views set forth in *A. N.* 4053 have been more or less anticipated by Professor Schuster," and "At the time of composing the paper in *A. N.* 4053 the writer was not aware of this."—Eds.]

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* Erroneously printed No. 4014 on p. 396 in our October number.

Tennyson's Science.

Gentlemen,—

In an interesting note "From an Oxford Note-Book" (*Observatory*, June 1906, p. 263) the writer criticizes a statement by Mr. Mackie, who considers that Spectrum Analysis is alluded to in the following lines from 'In Memoriam':—

> When Science reaches forth her arms
> To feel from world to world, and charms
> Her secret from the latest moon.

The fact that 'In Memoriam' was published in 1850, and that it was not until 1859 that Spectrum Analysis was applied to explain the constitution of the heavenly bodies, is conclusive against Mr. Mackie.

But I think that Mr. Mackie's curious mistake is due, not to a misunderstanding of Sir John Herschel's reference to the discovery of Neptune, but to a misinterpretation of the following passage, which occurs on p. 336 of vol. ii. of the 'Memoir on Tennyson,' written by his son:—

> In August (1887) Professor Jebb was with us, and he watched an eclipse of the Moon with my father, who said that, according to analogy, at least one of the planets belonging to each sun should be inhabited, though perhaps with beings very different from ourselves; and that the spectroscope was destined to make much greater revelations than it had already made, in charming
>
> Her secret from the latest moon.

A writer unacquainted with the history of the subject might easily, by this passage, be led to suppose that Tennyson, when he originally wrote the lines quoted above, was thinking of Spectrum Analysis.

Yours faithfully,

Leeds, 1906, Oct. 16. Chas. T. Whitmell.

OBSERVATORIES.

[Continued from p. 393.]

Frankfurt. *Epstein.*—Observation of sun-spots secured on 256 days. Three days of no spots were noted. The groups are arranged in latitude and longitude, and other information is given.

Geneva. *R. Gautier.*—The Plantamour equatorial has been in use, as in previous years, for small planets, comets 1905 *a* and 1906 *b*, this latter being discovered here by M. Schaer on Nov. 7. These have all been published in the *Astronomische Nachrichten*. Expedition to Majorca to observe the eclipse of the Sun, the chief object being to photograph the corona and prominences. Only partially successful owing to cloud. As a partial eclipse it was observed at Geneva, 22 photographs being secured by M. Schaer. Testing watches. Time-service. Meteorology.

GÖTTINGEN. *K. Schwarzschild.*—Expedition by the Director and M. Runge to Guelma to observe the solar eclipse. Abgesehen von Prof. Runges luftelektrischen Messungen erhielten wir zu Anfang de Totalität 16, zu Ende derselben 3 Aufnahmen des Flash-Spektrums, und während der Totalität 3 Aufnahmen des Korona-spektrums, sowie 8 Aufnahmen der Korona selbst (zum Teil mit Gelbschiebe). Namentlich die erste Serie von Flash-Aufnahmen, die auf einem mit der Hand ruckweise bewegten Film erhalten wurden, zeigt sehr schön alle Uebergänge zwischen Fraunhofer-spektrum und Chromosphärenspektrum. Durch photometrische Ausmessung dieser Spektren konnte ein Profil des Mondrands sich verlässegere Angaben über die Höhe der verschiedenen Gase in der Chromosphäre finden lassen. Das Koronaspektrum zeigt zwei deutliche ultraviolette Ringe. Die Korona bilder zeigen Strahlen bis zu 1° von der Mitte.

Prof. Ambronn made a number of measures of the Sun's diameter with the heliometer.

[To be continued.]

PUBLICATIONS.

PLANETARY SYSTEMS *.—It is now about fifteen years since the Director of the Armagh Observatory produced that exhaustive life of Tycho Brahe which will ever be of the highest interest in the history of our science. For it was Kepler's discussion of the observations of the great Dane and his firm confidence in their accuracy which led him to the discovery of the laws which regulate the planetary motions, and it was from these again that Newton deduced the law of universal gravitation on which scientific astronomy is founded. It is therefore with peculiar satisfaction that we welcome the present volume, dealing with the ideas and theories respecting the planetary systems from ancient times to that of Kepler. The author tells us in the Preface that erroneous notions and legends on the subject are still frequently found in books, even in those dealing specially with astronomy. One of these, which Dr. Dreyer believes has not been pointed out before, is that Alfonso anticipated Kepler to some extent by representing the orbit of Mercury as an ellipse, the fact being that the curve in question was constructed from a number of small arcs, and is nothing but the curve described by the centre of the epicycle of Mercury in Ptolemy's theory.

After an introduction on the earliest cosmological ideas, and a chapter on the early Greek philosophers, most of our imperfect information respecting whom and their conjectural views is founded upon the treatise 'Placita Philosophorum,' formerly, but erroneously, attributed to Plutarch, he passes on to the Pythagorean School, which first taught correct notions with regard to the motions of the heavenly bodies and the sphericity of the Earth, though in the latter point Parmenides may have had the priority. However, the teachings of the Pythagoreans accustomed men's minds to the idea of the spherical Earth, and after the time of Philolaus we hear nothing more of the crude notions of earlier ages. Coming to the greatest of the Greek philosophers (always called Plato, though his real name was Aristocles), Dr. Dreyer remarks that " there is absolutely nothing in his various statements about the construction of the universe tending to show that he had devoted much time to the details of the heavenly motions," but still the study of the 'Timæus' must ever be peculiarly attractive, and " Plato through his widely-read books helped greatly to spread the Pythagorean doctrines of the spherical figure of the Earth and the orbital motion of the planets from west to east." The homocentric spheres of Eudoxus, developed and improved by Kalippus, formed a distinct step in advance, speculation being now made to rest upon steadily pursued observation. Attention was first called in modern times to the views thus set forth by Ideler in two papers contributed to the 'Transactions of the Berlin Academy' for 1828 and 1830, but more fully by Schiaparelli in the Publications of the Brera Observatory, which appeared at Milan in 1875. Of course a whole chapter is devoted to Aristotle, the last great speculative philosopher who figures in the history of ancient astronomy. As in the case of Plato, his merits and defects are here impartially set forth. Heracleides and Aristarchus are put together as teaching, the first the rotation of the Earth, and the second (as would seem from a passage in Archimedes) its motion in space; but the latter hypothesis does not seem to have attracted much attention.

---

* 'History of the Planetary Systems from Thales to Kepler.' By J. L. E Dreyer, Ph.D. (Cambridge: the University Press.)

We have no space here to discuss Dr. Dreyer's chapters on the theory of epicycles and the Ptolemaic system, which held sway so long. He gives a very complete account of them, which will be read with much interest. The scholastic philosophy of the middle ages had nothing scientific about it; with one remarkable exception, that of Roger Bacon, all the mediæval writers regarded Aristotle as the last possibility of wisdom and learning in natural philosophy. The gap between ancient and modern astronomy was to some extent filled up by the Arabs; but we cannot find that they made any advance beyond Ptolemy in their ideas of the celestial motions. Nevertheless, Dr. Dreyer reminds us that their adoption of the calculus of sines greatly helped forward subsequent advance. When the revival of learning took place in Europe, a series of German astronomers arose who paved the way for Copernicus and Kepler. The first of these was Nicolaus de Cusa, born at Cues, a village on the Moselle, in 1401, who published his 'De Docta Ignorantia' in 1440, but whose astronomical ideas are best set forth, or at any rate hinted at, in a note found in his own handwriting on the leaf of a treatise issued somewhat later. The name of Johann Müller (commonly called Regiomontanus) is better known. He died at Rome, young, about three years after the birth of Copernicus. Dr. Dreyer's chapters on the latter, on Tycho, and on Kepler are replete with interest; but we must now content ourselves with saying of his book what Osiander did of that of Copernicus, "Eme, lege, fruere." It is indeed eminently one which should be read from cover to cover, but we conclude by quoting the final sentence:—"From Thales to Kepler philosophers had searched for the true planetary system; Kepler had completed the search; Isaac Newton was to prove that the system found by him not only agreed with observation, but that no other system was possible."

W. T. L.

STELLAR PARALLAX.—From time to time, when writing in these pages of Dr. Elkin's annual reports of work at the Yale Observatory, mention has been made of the investigation of parallax of stars being made there with the heliometer. Part 1, volume ii. of the 'Transactions of the Yale University Observatory' contains the results of this work on 163 stars, mainly of large proper motion, which is so full of interest that it may be well to consider the volume in detail.

Dr. Elkin completed in 1894 his observations for the parallax of the first-magnitude stars of the northern hemisphere, on which he had been engaged since 1885 [*], and before the completion of this he had begun to consider what should be the next class of stars to be attacked. It seemed good to make a preliminary survey of all rapidly moving stars to find those which had a measurable parallax. By 1895 measures had been made of 99 stars with proper motions exceeding 0″·5 a year and not fainter than 8·8. Later on, other stars were added, so that the list of stars which went to make up the observing list comprised 117 stars with proper motion over 0″·4, six red stars observed for the detection of a possible systematic error due to colour, β Cygni, Algol, Nova Persei, and some others.

It is not necessary to describe the method of observing in detail, but it may be said simply that the results depend wholly on measures of distance. In most cases two comparison stars were selected on opposite sides of the star whose parallax is under investigation. The observations and deduced results are given in 174 pages of the book—in general, one star to a page; and passing these by, we take leave to abstract from the tables in which these results are collected and tabulated in various ways, Dr. Elkin's results for First-Magnitude Stars before mentioned being included.

[*] See *Observatory*, 1903 May, p. 217.

1. Arrangement according to Proper Motion.

Average P.M.	0″·14	0″·49	0″·59	0″·77	1″·50
„ magnitude ...	3·8	6·3	6·7	6·5	6·2
„ parallax	+0″·019	+0″·032	+0″·059	+0″·039	+0″·109
Number of stars	21	39	45	46	22

2. Arrangement according to Magnitude.

Average mag. ...	0·8	3·8	5·6	6·7	7·6	8·3
„ P.M....	0″·61	0″·53	0″·63	0″·73	0″·68	0″·80
„ parallax	+0″·095	+0″·066	+0″·056	+0″·045	+0″·017	+0″·047
Number of stars.	10	29	33	34	31	36

3. Arrangement according to Parallax.

Average parallax	−0″·110	−0″·025	+0″·031	+0″·097	+0″·159
„ magnitude ...	7·3	6·4	6·8	6·1	6·3
„ P.M.	0″·55	0″·52	0″·62	0″·79	0″·97
Number of stars	7	29	66	44	17

About this third table the authors say that it emphasizes the connection between proper motion and parallax, none of the negative parallaxes corresponding to proper motions greater than 1″, and a marked progression being very apparent. "This table may also serve to indicate the number of spurious parallaxes belonging to the work. If, according to Newcomb, we regard all negative results as due to errors of observation, and likewise an equal number of positive values to balance these, it would seem that there are 90 stars of the entire list of 163 for which there is considerable presumption that the parallax values found are actual."

The fourth table arranges the results in order of R.A., and divides them into 6-hourly groups:—

R.A.	0h to 6h.	6h to 12h.	12h to 18h.	18h to 24h.
Parallax	0″·065	0″·033	0″·064	0″·035
P.M.	0″·73	0″·62	0″·83	0″·55
Number of stars	42	40	40	51

The agreement of the first and third groups and of the second and fourth suggest a temperature-effect, but the authors think that this cannot be shown.

The final table gives the average parallax of the stars in the list of Spectral Types I. and II.:—

	Type I.	Type II.
Average parallax	+0″·065	+0″·058
„ P.M.	0″·42	0″·67
Number of stars	13	81

To this brief summary of the contents of the volume may be added a short reference to some special observations made to determine the effect of the colour of red stars on the measures. This was done by making measures of five highly coloured stars relative to comparison stars when they were on the zenith and when they were at rather large hour-angles east and west of the meridian, the underlying assumption being that any perceptible difference in the mean refrangibility of the light of two stars would be a function of the hour-angle. In the end the result showed that there was a discernible colour-effect conforming with theory, the mean light of the red star being refracted less than that of the comparison stars; but the effect on the measures is presumably well within the probable error, and is not likely to vitiate the parallax. "That the colour-effect is not greater rather tends to confirm Sir David Gill's opinion that the observer's tendency is to bring the similarly coloured parts of the star's spectra rather than the brightest parts into coincidence."

It may seem inadequate to compress the record of ten years' hard and skilful work into these few lines, but they are, perhaps, sufficient to call attention to a series of results that will, without doubt, be of much service to investigators. We offer congratulations to the authors, Dr. Chase, Mr. M. F. Smith, and Dr. Elkin, on the completion of their task.

NOTES.

Minor Planet Notes.—The following new planets have been discovered:—

Letter.	Date, 1906.	Discoverer.
UM	June 14	Metcalf.
UN	21	Wolf.
UO	July 30	"
TY[a]	Mar. 27	"
UP	Aug. 13	Kopff.
UQ	22	Wolf.
UR	22	"
US	22	"
UT	22	Kopff.
UU	27	Wolf.
UV	28	Kopff.
UX	Sept. 12	"
UY	12	"
VA	17	"
VB	Sept. 18	Kopff.
VC	18	"
VD	18	"
VE	15	Metcalf.
VF	24	Wolf.
VG	24	"
VH	24	"
VJ	26	Kopff.
VK	26	Wolf.
VL	24	Metcalf.
VM	24	"
VN	Oct. 8	Kopff.
VO	8	"

Planet 480 (1901 GL = 1905 QJ) has been named Hansa.
" 526 (1904 NQ = 1901 HB) has been named Jena.
" 463 (1900 FS) has been named Lola.
" 481 (1902 HP) has been named Emita.

UW is identical with 66 Maia, UZ with 408 Fama.

SF is identical with 488 Kreusa, and Planet 469 (1901 GB) is also identical with 488 Kreusa. The number 469 will be assigned to 1901 GE. It is further possible that 1896 CR may also be identical with 488 Kreusa. A. C. D. C.

The approaching Return of Halley's Comet.—At the next return of Halley's Comet (when, according to Pontécoulant's calculations, it will reach perihelion about the middle of May 1910) the radius vector will be much greater than at the last appearance in 1835. At the instance of Hofrat Weiss, Director of the Vienna Observatory, Dr. Holetschek has undertaken to investigate the conditions of its visibility, and the results are embodied in a paper published in a recent number of the 'Sitzungsberichte' of the Vienna Academy of Sciences. Pontécoulant had remarked that, in consequence of a special combination of perturbations, the period between the last and the approaching return is the shortest of any since that of 1531, amounting to only 74½ years; the last perihelion passage, in fact, having taken place on the 16th of November, 1835, and the next being due on the 16th of May, 1910. Dr. Holetschek has calculated its positions and distances from the Sun and Earth at the next four oppositions to the Sun, about the end of 1906, 1907, 1908, and 1909. At the first of these (December in the present year) a search he thinks, would be hopeless on account of the great distance of the comet. The probability of seeing it at the end of next year would be almost infinitesimally small, but would be considerable when near opposition in the latter part of 1908, whilst towards the end of 1909 it may confidently be expected to be within observing reach. In January 1910 its brightness will probably be as great as when first seen at the last return, in August 1835, and in the second half of March it will probably become visible to the naked eye. W. T. L.

Universal Time.—It has been enacted by the Council at Port Louis that the Mean Solar Time of longitude sixty degrees east of Greenwich shall be deemed and is declared to be the Standard Time throughout Mauritius and its Dependencies, except the Chagos Archipelago, where the Mean Solar Time of longitude seventy-five degrees east of Greenwich shall be deemed and is declared to be Standard Time.

The enactment in both places is to come into force on 1907 January 1. The Chagos Archipelago is a group of eight islands in longitude about 72° East and latitude 3° South. This Archipelago is the first to take up the time of the zone five hours east. As was announced in our September number, the Colony of Seychelles has already adopted the use of the time four hours fast on Greenwich.

The Seventh Satellite of Jupiter was re-observed by Dr. Perrine at Mount Hamilton on 1906 September 25·9962 G.M.T. Position-angle 119°·1, distance 2578″. This appears to be the first observation of this satellite in the present season.

M. J. P. Lagrula, D.Sc., formerly of the Observatory of the University of Lyons, has been appointed Director of the University of Quito.

The first Meeting of the forthcoming Session of the Royal Astronomical Society will be on Friday, November 9. The next Meeting of the British Astronomical Association will be on Wednesday, November 28.

From an Oxford Note-Book.

A great astronomical career is drawing near its official end. It is superfluous to refer to expectations for the future at this moment; our thoughts are with the chapter—the long and prosperous term of official activity—which is just closing. Sir David Gill may be in England at this moment, for a newspaper cutting sent by a kind correspondent brings news of a farewell banquet to him at Capetown on some date of which the sender has removed all traces. At this banquet eloquent things were said, in all sincerity and with all justice, of Sir David Gill's twenty-seven years' work in South Africa; though the speakers seemed a little divided in opinion whether to direct attention chiefly to the purely scientific or the more utilitarian side of it. "The pursuit of knowledge for the sake of knowledge," said Mr. Merriman, "that was the Science to which their distinguished guest had devoted his whole life"; and he carefully distinguished it from "the science which had given them a great many conveniences and a great many nuisances; the science which had given them the telephone and the electric cable; and had also given them the motor-car—(laughter) —and other nuisances of civilization." To Mr. Merriman followed Mr. Walton, who declared that we "could not escape the fact that science was becoming more and more associated with the daily problems of our life, and was becoming not the handmaiden, but the director of industry"; and who, "speaking as a South African, felt that in one branch of Sir David's work they could all appreciate fully the tremendous service he had rendered to South Africa in the Geodetic Survey."

Surely no more significant tribute could have been paid to Gill than the combination of two such champions of eternally opposing views to do him honour. It is no wonder they found it difficult to single out one feature of his work for special praise; for what a succession of restless activities have been crowded into that twenty-seven years! Whether we think of Gill as an actual observer with the heliometer whose observations have challenged comparison ever since, or as continually demanding more and more observations from others, and raking up those of past observers to put them in order; whether we regard him as toiling incessantly at the erection of a great observatory or as a leader in general enterprises such as the Astrographic Chart, the Geodetic Survey of South Africa, or the formation of a South African Association for the Advancement of Science; whether as a voluntary exile in a remote Colony, or as a man you might meet in London or Berlin or Paris any day of the week: no matter which department of work and devotion possible to an astronomer we choose, we do not find Gill wanting. Prof. Kapteyn will, I feel sure, forgive my recalling for a moment the few words he said in 1902 on receiving the Royal Astronomical Society's medal for the Cape Photographic Durchmusterung. They were said to a semi-private gathering, but from the earnestness with which he spoke I feel sure he would repeat them to the whole world. They were to the effect that if that great question which we all have to resolve, whether our lives shall achieve something—if that question should be happily answered for him in the affirmative, he owed it to Gill.

There are perhaps two things Gill has not done. He has not observed an eclipse and he has not been to the United States. What will be his future action in these two respects? He may elect to stand by his old habits; but if he goes to America—my! won't he wake up their sleepy old country!

It is not often that anyone is willing and able to do a year's hard work, if he could only find something to do, but such cases are not unknown; and this is perhaps sufficient excuse for mentioning instances where a valuable result is sure to be attained on the chance that the paragraph may meet the eye of some one with the requisite leisure. In a recent letter from Mr. S. A. Saunder there was the following paragraph, which I have obtained his permission to quote, though it was written without any idea of publication:—

"There is a great piece of work waiting for some one to do on the Moon's Physical Libration. Hayn has given a new and much more complete discussion of the theory and has applied it to his own observations and to Hartwig's. But Schlüter's observations form a better series than these two together and certainly ought to be re-reduced by Hayn's methods. I have written to Hayn and Franz about it, but neither is able to undertake the work. It would, I think, mean a year's hard work for anyone, but the result would be a considerable increase in our knowledge of the Physical Libration."

A VALUABLE remark is made in a paper on photographic measures at Pulkowa of Neptune's satellite by S. Kostinsky and Mdlle. L. Timoféeff, concerning the possible influence of one photographic image on another adjacent. When the plate is wet (during development &c.) there are slight depressions at the places of the images; and the authors remark that if two images are very close and very different in size, one may influence the other by some lateral displacement of the swollen film, and the difference in size may produce odd anomalies. The remark is purely tentative at present, though it seems to offer an explanation of some anomalies met with in measures of Neptune's satellite. The authors suggest experiments on double stars and perhaps on adjacent lines in the spectrum of different intensity. There is apparently opportunity for an interesting research here.

Reference has been already made in these notes to the possibility of a connection between latitude variation and earthquake phenomena. A statement by Dr. Omori to the Astronomical Society of the Pacific (who have been compelled by tragic events to take a grim interest in earthquakes) exceeds in definiteness anything I have as yet seen or heard of the kind. He says :—

> From an examination of the mean monthly values of the latitude of Tokyo, I have found that all the destructive earthquakes of recent years in Japan occurred exactly or very nearly when the latitude was at a maximum or minimum. (*Publ. A. S. P.* xviii. p. 240.)

According to tradition, the late Prof. Cayley thought that arithmetic was taught all wrong in the schools. The ideas were, in his opinion, not developed in logical sequence. His own notion was to begin with the quadratic equation, which introduced at once both real and imaginary quantities: on which secure foundation the superstructure could be confidently erected. There are other stories about Cayley of similar significance, tending to show that he had quite forgotton what was really elementary. But an incident which came under notice the other day suggests that perhaps there is more of convention than we suppose in the generally assumed order of ideas. A lady heroically read through an investigation containing rather more mathematics than she was accustomed to, and in her determination to understand every step made the discovery that $\cos 2\theta = \cos^2\theta - \sin^2\theta$. She started from a statement that sin (Sun's altitude) $= l\xi + m\eta + n\zeta$!

From the *Daily Mirror* of October 17 :—

> After Sir George Airy, who, as Astronomer Royal, was for fifty years connected with Greenwich Observatory, ~~it is~~ proposed to rename Devonshire Road, Greenwich, "Airy Road."

The same of October 3 :—

DOES THE EARTH REVOLVE?

Extraordinary Theories put forward by an Engineering Authority.

> "The explorer who first arrives at the North Pole will do so in some form of airship, and will probably discover the foundations of the earth, and dispose once and for all of the ridiculous and absurd theory that the world is revolving in space."
>
> This is the startling theory put forward by no less an authority than the editor of 'Civil Engineering.'
>
> "There are several reasons," he says, "for supposing the Earth does not revolve. If it did any man wanting to go to America would only have to go up in a balloon and wait till America came along. Then, again, a point on the equator is said to be revolving at the rate of seventeen miles a minute. If that were so we should be flung into space immediately, since the tangetical motion would be much greater than the force of gravity.
>
> "Astronomers can show no evidence to prove that the Earth revolves.

"The world is resting on a solid foundation. If the supports, when discovered, are followed up they might possibly lead to the discovery of other hemispheres, but that I cannot say."

A CORRESPONDENT has kindly sent me a little book* of 63 pages, privately printed, recounting autobiographically the adventures of a sailor who went to sea as a boy of fourteen in 1804, and who seems to have had a pleasant enjoyable life, marred only by a dark period following the loss of every shilling he possessed in 1828. The following extracts will interest astronomical readers:—

> But the Captain had over-estimated our rate of sailing, in consequence of which we found the reckoning to be some 5° in advance of the ship, causing us to heave to for two nights, sailing with all sail set during the day until we sighted Antigua. Just before making the land we hailed a Liverpool ship, also outward bound, and found that her longitude differed 9° from ours, so that if our reckoning had got 5° ahead hers was 4° astern. So much for dead reckoning in those days [*i. e.* in 1811]. Neither of the ships had a chronometer on board. (Pp. 24–25.)

Nevertheless some extraordinary voyages were made:—

> Sailed for London on 16th November [1821], when the frost was very severe and the hempen cables so hard that we were not able to let go an anchor day or night. . . . After landing the pilot at Father Point at 10 P.M. on November 18, it came on very thick weather, so that in the river we never saw land again. Heard the breakers on the beach near the south-west point of the island of Anticosti and sounded in 55 fathoms, wind increasing to a gale and veering to the N.N.W. with a heavy snowstorm . . . . which continued for 36 hours, the ship and rigging becoming covered with snow and ice, the sails being frozen to the yards. The snow abating we bore up for the passage between Cape Ray and St. Paul's Island and went safely through in the night without seeing either, although we had seen no land or the sun since parting with the pilot. . . . The passage across the Atlantic was a rough one with very thick weather in approaching the English Channel, so that in the fog we had to grapple our way up the Channel by the lead until we made Beachy Head. This was the first land we had seen since leaving the pilot at Father Point in the River St. Lawrence, and the deep sea lead was our only chronometer. (Pp. 58, 59.)

WHEN chronometers began to be used in the Merchant Service, the proper care of them was not well understood, and many tales were told of the alarm of masters of ships lest they should forget to wind them up, and of the schemes resorted to for preventing this happening. One would order the words "Remember the chronometer" to be painted on a board and set up in some conspicuous part of the cabin; another would have a card bearing similar words placed by the steward on his plate every morning at breakfast; but a more effectual method was said to be to give the man at the helm a glass of grog every morning at four bells (10 A.M.), known as the grog trick at the helm. A tale used to be told of a perplexed master who, on asking a brother mariner what he should do if so unfortunate as to let his chronometer run down, received for reply that the only sure method would be to put the ship about, run back, and set his chronometer by Greenwhich Church clock. Although chronometers were at first not well understood in the Merchant Service, I believe that more masters and mates of ships in the foreign trade could then take satisfactory lunar observations than in later times, and such observations were very useful for checking chronometers on long voyages. (P. 59.)

---

* Life in the Merchant Marine One Hundred Years Ago, by Samuel Jefferson, Master Mariner. (C. North: Blackheath, 1905.)

No. 377.] DECEMBER, 1906. [*Price* 1s.

THE OBSERVATORY

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H. P. HOLLIS, B.A., F.R.A.S.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1906 November 9.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder. One hundred and sixty-five presents have been received since the last Meeting. Among others may be mentioned Vols. 16, 17, and 18 of the National Edition of the Works of Galileo, presented by the Italian Government; two copies of the Strasburg Zone, −2° to −6° of the Astronomische Gesellschaft Catalogue—one presented by Prof. Becker, the other by the Gesellschaft; and a book entitled 'A System of Applied Optics,' presented by the author, Mr. H. Dennis Taylor.

We have also 78 enlarged reproductions of Astrographic Chart plates from the Royal Observatory, Greenwich, and 96 from the National Observatory, Paris. Also two enlarged transparencies and a lantern-slide from negatives of the total solar eclipse of 1905 August, taken at Alcala, Chisvert, Spain, by Count de la Baume Pluvinel.

The President. We have with us to-night Sir David Gill, who, though he is still the Director of the Cape of Good Hope Observatory, has, I believe, returned to England for good, so that we may hope to see him frequently at our Meetings in future. I will ask him to tell us something about the work he has been carrying on at the Cape.

Sir David Gill. The last special work we have been engaged upon resulted from a request of Prof. Lewis Boss. He wrote me towards the end of 1905 that he was starting upon the formation of a catalogue of stars, and to complete it he wanted recent observations of about 1100 stars, and he wished to know if we could undertake the task. It was essential for his purpose that we should get these stars observed and reduced before the end of October 1906. So we set about them, and he has now got the places of the 1130 stars in his hands, and he thanks me for what has been the most rapidly produced catalogue that has ever been made.

Simultaneous with the construction of that catalogue we have been working at the new transit-circle. You may remember, in connection with that, we had a lot of trouble with the installation of our meridian marks. The geological structure underlying the site of the Observatory is upon rock which is disintegrated to the depth of some 30 feet. I was anxious, for several purposes I had in view, to obtain an azimuth reference of very great stability. To do this we had to go down to the solid rock, which, presumably, affords means of establishing azimuth marks of great constancy. And then, however solidly these underground marks might be fixed, it was necessary to have the means of transferring the marks vertically to the surface, so that they would be visible above ground. For this purpose we had to make pits down to the solid rock, line the pits with iron to exclude water, and fix our marks at the bottom of these pits. The marks, thus practically fixed to the solid rock, are the optical centres of object-glasses.

The north and south overground marks observed by the transit-circle are viewed through lenses of about 300-feet focus, the marks being in the principal foci of these lenses, so that the image of the mark is formed in the same focal plane of the transit-circle as the images of infinitely distant objects.

It therefore remained to devise means by which the centres of each of these two long-focus lenses and each of the two marks should be centred over the corresponding underground marks at the bottom of the iron-lined pits.

The slides carrying the long-focus lenses and marks were fitted with small circular apertures about half a millimetre in diameter. These apertures, when illuminated by light from a small electric lamp thrown upon a white plane inclined 45° to the vertical, were in the principal foci of the underground lenses. A pool of mercury placed under each of the underground lenses reflected back the light from these illuminated apertures, so that an image of the illuminated aperture is formed in the same focal plane as the illuminated disc itself. On the same slide with the illuminated disc is fixed a single thin metallic strip or wire stretched in the direction of the meridian. The underground lens is fixed so that its optical centre is vertically below the middle point between the centre of the wire and the centre of the illuminated disc. Therefore by moving the slide E. or W., the wire can be made to bisect the image of the disc, and thus the long-focus lens or the mark can be always readily brought exactly vertically over the corresponding underground mark, or at least always to a constant distance E. or W. of it.

We have found in this way that when the adjustment is made previous to each observation for azimuth, the angle (nearly equal to 180°) between the north and south marks remains absolutely constant within the limits of observation. But as there are scales attached to the slides which carry the 300-feet focus lenses and the marks, we can ascertain what would have been the changes in the relative azimuths of the marks if we had trusted entirely to the stability of the piers which support the lenses and marks, and we find that since the marks were erected they have changed relatively to each other by no less than six seconds of arc. That is to say, the underground marks preserve an absolutely constant relative azimuth, whilst the overground marks, if not daily adjusted to coincidence with the underground marks, would have varied six seconds of arc relative to each other.

When the observations of circumpolar stars have been finally reduced and discussed, we shall then know whether the absolute azimuths of the marks have remained equally constant.

We have found a very regular change in the azimuth of the transit-circle itself between sunset and midnight, always of the same sign, amounting to about half a second of arc in summer to less than a quarter of a second of arc in winter. There is much that is new in the instrument, such as in the means of observing by a clock-driven wire, the plan of the mounting, and so forth; but I have made a complete illustrated description of the instrument and of all the instruments of the Observatory, which is now in the hands of H.M. Stationery Office for estimate, and, I hope, for printing, so I need not detain you with further description.

With regard to the spectroscopic work, we have been very busy taking an immense number of spectra in the evening and in the early morning for determining the solar parallax by displacement of the spectrum-lines in the line of sight. We have got a large number of observations and fairly concurrent results, though I think it is still possible to improve these. We got variations from the mean, sometimes in a number of lines in the same spectrum, ranging over five kilometres per second. I do not see why it should be so big. We have had our spectroscope maintained within one-tenth of a degree Fahrenheit for days together, and it is certainly not there the fault arises. We have struggled to get greater perfection. There is no doubt that the apparent disagreements are very largely due to errors in the absolute wave-lengths of the lines employed, and I have no doubt but that when we come to the final discussion of the solar parallax from these observations, by comparing displacement of the same lines at different epochs, when we have only to deal with change in the displacement of the same lines, and the absolute wave-length does not come into the problem at all, we shall see then how far these errors depend upon composite lines in the stars of which we know nothing, and so forth, and how far they depend upon accidental errors of pointing. My own belief is that Hartman has really got at the kernel of the best method of measuring displacement in the line of sight, by the method he has recently described in the observations of the Potsdam Observatory, whereby he compares the spectrum of the Sun directly with the spectrum of the stars. He first takes a photograph of the Sun with the iron lines, and the spectrum of a star with iron lines, surperposes the images ef both, and measures the displacement of the iron lines. In that way I think a great advance can be made.

With the heliometer we have, of course, been making every year regularly measurements of the major planets relative to comparison stars. I have now with me the MS. of this work up to the end of last year ready for publication.

Another curious point about which I am also going to publish something is that in discussing the observations of the Sun we have found that at the instant we changed from the eye-and-ear to the chronographic method of observation we got a distinct change of personality in the observers. And it is not applicable to one observer; it is applicable to all the Cape observers. If this is general elsewhere it will have an important influence on the value of precession derived from observations of the Sun when made of old by the eye-and-ear method and made now by the chronographic method.

There are ever so many other new things to say. But I am going to write about them afterwards at leisure. There is a paper to follow by the new Chief Assistant of the Royal Observatory at Greenwich which excites my curiosity, and I am anxious to hear that.

Mr. A. S. Eddington gave an abstract of his paper on "The Systematic Motions of the Stars," the substance of which is as follows :—Of late years astronomers who have investigated the proper motions of the stars have generally assumed that if we consider a sufficient number of stars their true motions are at random, and on this assumption the direction of the Sun's motion has been calculated; but Prof. Kapteyn has lately examined the proper motions of the Auwers-Bradley Catalogue to find out whether this assumption is approximately true, and concludes that it is incorrect. Relative to the Sun he found two "favoured" directions of motion instead of one, and therefore suggested that there are two systems or "drifts" of stars, and that these two drifts are in motion relative to one anather. In this paper the attempt is made to subject Prof. Kapteyn's theory to a quantitative test by examining the Greenwich-Groombridge proper motions. The following reasons, among others, seemed to make this desirable. The Bradley stars are all bright stars, of those of the Groombridge Catalogue a large number fall between the seventh and ninth magnitude. The 2500 stars of Bradley's Catalogue cover most of the sky; the 4500 stars of Groombridge are all within 52° of the North Pole.

A "drift of stars" is defined to be a system of stars whose velocities relative to some system of axes are quite haphazard. The velocity of the drift is the velocity of the aforesaid system of axes. The "peculiar" velocity of a star is its velocity relative to that system. The ordinary hypothesis on which discussions of the solar motion are based regards the universe as forming one such drift. It is sometimes further assumed that this is at absolute rest, but, of course, referred to the Sun it is in relative motion. A small region of the sky may be taken which can be considered approximately plane, and the stars in it supposed to belong to one drift. Ignoring velocities in the line of sight, a curve may be obtained by drawing the radius vector in any direction proportional to the number of stars having proper motion in that direction.

[A diagram was shown of curves drawn on this principle, one for each of seven regions of the sky into which the area covered by the Groombridge Catalogue was divided. These were discussed separately, and it appeared that in each case these were more consistent with the hypothesis that the stars fell into two drifts than that they were all drifting in the same direction.] Mr. Eddington continued:—It must not be supposed that I claim that two drifts represent completely the phenomena of the distribution of proper motions, and that the discordances are necessarily either errors of observation or the purely accidental departures which the theory of probabilities allows. Local irregularities must be expected. What is claimed for the two-drift hypothesis is that it is a good first approximation, whereas the one-drift hypothesis is no approximation at all.

The analysis of the figures for each of the seven regions, except one which contained too few stars, showed the directions of the two drifts in each. These directions were plotted as great circles on a globe taking the centre of each region as origin, and it was found that the great circles for drift I. (produced backwards) all passed close to the point R.A. 18^h, Dec. $+18°$, so that the stars of drift I. have evidently a common velocity relative to the Sun away from this point. The directions of drift II. do not define a point with the same accuracy. The best position for the apex of this drift is given by R.A. $7^h\ 30^m$, Dec. $+58°$. The positions of these apices agree very well with those found by Kapteyn. The velocity of the first drift relative to the Sun is very much greater than that of the second, the ratio being about 17 : 5.

There remains to consider whether the average distance of the drift II. stars is different from that of the stars of drift I. The first system might be a cluster of stars containing the Sun moving relatively to the other system, which might be a background of more distant stars. This was investigated by using the *magnitudes* of the proper motions, and the main conclusion arrived at was that there is no appreciable difference in the mean distance of drift I. and drift II. stars.

The Astronomer Royal expressed his warm appreciation of this excellent piece of work.

Sir David Gill. It is difficult to say much by way of criticism, as I received a copy of the paper only this morning, when I had a great deal to do. But I saw enough to induce me look forward to the meeting with the deepest interest. The fact that Mr. Eddington deals only with direction of the proper motions and does not encumber himself with any hypothesis as to distance of stars in his discussion shows very clearly that we have nothing to look for in the way of fundamental error, except such error as may possibly arise in connection with imperfection of the early observations, and that seems to be fairly small.

The great point that has made me so enthusiastic upon this subject is the fact that my friend Kapteyn's work has been so splendidly confirmed by this discussion founded on the proper motions of stars in a limited part of the sky; and I look forward to rapid development of our knowledge of the constitution of the universe from the fact that the first steps have been taken on a sound basis.

When Prof. Kapteyn was out at the Cape I suggested to him that we have two series of photographs of the catalogue plates, the one originally made, and afterwards a second series with a time interval sometimes over 10 years.

He picked out 16 plates in the Cape Zone, which, you remember, is between Declination 40 and 52 degrees, and we got an old and a new series each of 16 plates, which were fairly symmetrical in R.A., and separated in epoch by about 12 years; and we measured both sets and sent the results to Prof. Kapteyn. I hope these will afford means of strengthening his investigation with regard to this great universal feature of the constitution of the Universe.

In reading this paper of course I was struck with the fact of the probable existence of a third stream. I have no doubt there are other incidental streams or connected subgroups of proper motion also to be met with. But we are a long way from settling what these are to be or where they are to be. It is a great satisfaction to me, and I am sure to every astronomer here, to find that we are in a fair way to solving one or two big leading problems of the Universe.

Prof. Turner. I should like to acknowledge the courtesy of Mr. Eddington and the Secretaries in sending me an advance copy of the proof of this paper. As it happens, I have not had an opportunity to do more than glance at it, but even that enabled me better to follow Mr. Eddington, and I am sure that the Fellows generally would appreciate an extension of the system of advance copies, if the Secretaries can see their way to it. (Applause.) I should just like to ask Mr. Eddington a personal question, whether he has exchanged any correspondence with Prof. Kapteyn on this subject? Whether he has had any criticism from him? Then, also, is it not possible that the proper motions may be roughly all of the same magnitude in the two systems owing to the method of selection? Would not that fact give an apparent equality of distance to the two systems?

Prof. Dyson. I should like to offer Mr. Eddington my congratulations. I had the pleasure of reading his paper, and, having read it somewhat carefully, must say I was struck by it very much indeed. Everybody who has worked on solar motions has experienced difficulties and anomalies which he could not altogether fathom. I was quite certain that in the main what we call solar motion was a real phenomenon; it was something that really existed, because, however we divided the stars, however we grouped them, we always got approximately the same thing; but still there were bigger differences than we had any reason to expect as the result of accidental or systematic error. When Prof. Kapteyn's paper was published I thought we had the key to the problem, and I was delighted to get it again from Mr. Eddington in numerical form. I have only seen a short paper of Prof. Kapteyn's in the Brit. Assoc. Reports, where most instructive results were obtained graphically and which opened the field for the more quantitative discussion which Mr. Eddington has made. The result that the stars are divided nearly equally among two streams is very remarkable, and it explains why people working at solar motion and in ignorance of these streams have obtained such consistent results. I hope Mr. Eddington will be able to continue his work with the still fainter stars of the Carrington Catalogue.

Mr. Hinks. I, too, have to thank Mr. Eddington and the Secretaries for an advance copy of his paper. On reading it I was immediately struck with the sentence: "These anomalies are entirely explained if we suppose the *visible universe* to be composed of two drifts . . . ." If I understand aright, all the proper motions together, large and small, give the drift very strongly; but if all the proper motions below 5″ per century are taken out, the signs of drift are, if anything, stronger. I believe that Kapteyn has said that stars with this motion are uniformly distributed over the sky; but that if we go below 5″ per century there is an aggregation on the galactic plane; to which Newcomb objects that the smaller proper motions are quite unreliable. So I should like to ask whether the new accumulations of proper motions determined at Greenwich confirm Kapteyn or Newcomb. One would like to know whether any of these proper motions really belong to the Milky Way proper. I cannot help feeling that the Milky Way star-clouds are probably a long way beyond our present means of determining proper motion, and that probably the observed drifts may really belong to the one particular star-cloud in which the Sun happens to be. And I should like to offer my humble congratulations to Mr. Eddington on his maiden paper—surely one of the most remarkable we have heard.

Mr. H. F. Newall. I have listened with great admiration to Mr. Eddington's paper; and having heard it I feel inclined to encourage every astronomer who is engaged in work with the slit spectroscope to destroy his apparatus in order to induce somebody to invent a *wholesale* method of finding velocity in the line of sight.

The President. I think it would be an admirable thing to get Prof. Kapteyn's views, and if he would come personally to the Meeting so much the better. We might then reopen the discussion on Mr. Eddington's paper.

Mr. Eddington. I feel grateful to the speakers who have taken part in the discussion for so friendly an attitude towards this paper. I have not corresponded with Prof. Kapteyn; doubtless it would have been a great advantage to me to have done so. Sir David Gill referred to accidental error. There is an advantage in this method that it is fairly independent of accidental error. The effect of small accidental error is simply to alter the value of the unit in which the drift velocity is expressed, but we have not been at all concerned with the actual numerical value of the unit. With regard to Prof. Turner's suggestion as to the effect of the limit of 5″ a century, I did not use that limit in the analysis: I used all proper motions, however small, of course knowing that the directions of the small proper motions were not very trustworthy, but relying on the fact that accidental error does not affect the results. In deducing the mean distance of course I had to reject very great proper motions, but I adopted the rule of rejecting those greater than four times the mean, so I do not think that could have had an artificial effect in making the two drifts appear at the same distance. Mr. Hinks's question of the distribution of the stars having proper motions greater than 5″ a century is one I really had little to do with. I think Messrs. Dyson and Thackeray give a table showing their distribution in the Introduction to the Groombridge Catalogue. I may say I have derived great benefit from Mr. Thackeray's advice and suggestions throughout; he has had a large share in keeping me from wandering off the right track. From him I learnt the importance of avoiding any assumptions as to the distances of the stars.

The President asked Prof. Larmor to give a description of the paper presented by himself and Major Hills.

Prof. Larmor. I must first state that the blame for this communication does not rest with me. Had it not been for Major Hills' persistence there would have been no such paper. The history of it arises from a conversation with Major Hills last summer about the matter of the Chandler Period of Rotation of the Pole and the irregular wanderings superposed on it. I expressed the view that they had been observed over a long period of years, and had given a long series of tortuous curves, and it was time something was done with them. I thought it would be possible dynamically to ascertain something about what they might mean in a graphical way. Major Hills undertook to do the graphical part, if I did the other part; and he held me to the arrangement. The first attempt at giving a theoretical explanation of what was meant by the Chandler Period being 428 days instead of the 306 days indicated by Euler's theory of free precessional spin—the first light on the matter—was made by Newcomb, who pointed out that if the Earth were yielding, were elastic, then, it being very nearly spherical, the bulging of it due to the centrifugal force would be an important feature in determining the period. The free precession arises from defect from sphericity; but the Earth is so nearly spherical that the elastic bulging due to changed centrifugal force counts for something. The matter was mathematically investigated, after Newcomb, by Mr. S. S. Hough, in a valuable memoir in the 'Philosophical Transactions' which attracted my attention some ten years ago. At that time I partially generalized Mr. Hough's results in a paper which appeared in the 'Cambridge Proceedings'; and the conclusion amounts to this, that with an elastic Earth, elastic in any manner however complex, the actual free period is what the free period would be if the centrifugal force were entirely taken off; if we thus got the original shape, on the hypothesis of perfect elasticity, and it remained rigid at that, there would be a Eulerian period that would be the 428 days of Chandler. It is not hard to prove that the precessional effect of the elasticity of the Earth is as stated, while other disturbing causes can hardly be regularly periodic; and in fact the path of the Pole does go round in 428 days with wonderful average regularity. This free period therefore exists; there is something definite of that kind. It is possible dynamically to subtract from the path of the Pole the free precession with this period, which is a perfectly regular thing, and see what the residue may mean; and that is what we tried to do. The method can be stated in a much shorter way than the way which occurred to one at first. As a matter of fact, it occurred to me through some analogous dynamical problems relating to the change in the spectral lines of a molecule produced by a magnetic field, now well-known as the Zeeman effect. The same method of rotating coordinates, operative in that problem of physics, is also available graphically in this matter of pure dynamics. The upshot of it is this, that as the Earth is very nearly spherically balanced, the axis of rotation is very nearly fixed in space (abstraction is here made of the forced astronomical precession); and you can trace the wanderings of the pole of inertia or figure of the Earth round that fixed axis of rotation. The relative motion will be the same, whether you consider the pole of rotation as moving with respect to the pole of figure or *vice versa.* Well, if there were no irregularities at all the Pole of figure would describe its circular precession round the Pole of rotation in a perfectly regular manner in 428 days. Superposed on that is the fact that there are moving masses all over the Earth's surface, and these are continually changing the locality of the Pole of inertia on the Earth's surface. So that if by a graphical method you take off a persistent rotation with angular velocity corresponding to 428 days period, you have left the vagaries of the Pole of figure relative to the fixed Pole of rotation, which arise from meteorological and other causes. The main effect of the displacement of material over the surface, considered as loose independent matter carried by the Earth, in altering the Pole of figure is due to its inertia, and may be represented as the result of centrifugal force of the altered positions of the masses. The hodograph of the residual diagram is then constructed; and inspection of the very tangled diagram thus obtained should give some notion as to changing distribution from time to time of the moment of inertia of the movable masses on the Earth's surface, or it may be inside it. Well, we have four diagrams which thus give by their radii vectores the changing values of the torque (or couple) producing the irregular part of the vagaries of the Pole, when the regular free precession is eliminated; and we think any discussion as to causes,—melting of the polar ice, seasonal barometric changes causing accumulation of air, or seasonal changes in the ocean's level—we think any discussion of those things which must, I suppose, be the cause of the irregular part of the motion, would have to take account of the modified diagram—that is, of the diagram of force thus derived from the diagram of observed displacement. As time presses, the only additional thing is to show what these curves of force are like; and Major Hills has some pictures of them. It will be seen that our expectation that they would show a marked annual periodicity, which formed a main motive for the undertaking, has hardly been fulfilled, except in the case of the original Chandler curve, which was made up partly on that hypothesis.

Major Hills. Prof. Larmor has told you that my part of the work of this paper was the graphical analysis of the movement of the Earth's Pole, and I have a portion of one curve here which will illustrate the method. For the sake of simplicity, I take only three years—from 1900 to 1903. The first curve shows the motion of the Pole as deduced from the international observations by Prof. Albrecht. Now the first step in the graphical analysis is to take out the Chandler period of 428 days, and instead of referring the Pole to the fixed axis in the Earth, to refer it to an axis revolving in 428 days, and the result comes as in this second diagram. You will notice at once that, as far as these three years are concerned, there is no evidence of any further period; that is to say, no evidence of a yearly period, for instance. It will be seen in the printed paper, if it is printed, that analysis of the curve of the early observations does appear to show a yearly period, but this period disappears when we take more accurate later curves which are reduced from a more complete and more systematic series of observations. The next step in the process is to convert this from a curve of direction into a curve of force, and to do that we take a hodograph of that curve. In this the radius vector of any point represents the direction and velocity of the movement of the Pole at the corresponding date. This curve is still referred to the revolving axis, and therefore the last step in the analysis is to untwist this curve again so as to refer it to an axis fixed in the Earth. The next diagram shows the forces acting on the Pole to produce its observed movement. You will see at once in this diagram there is no periodicity, it is quite irregular. As to discussion on the exact forces which produce this movement, that must be left till we get a little more accumulation of knowledge. Later observations, and therefore presumably better ones, seem to show that evidence of any actual period in these tort diagrams disappears entirely, and we are left with a quite irregular curve.

Sir David Gill. The paper just read seems to point to observations of change of latitude as of future importance in world-wide meteorology, because change of latitude is but an expression of the effect of broad meteorological phenomena, such as the transference of masses of water from one point of the Earth to another or from sea-level to the tops of mountains in the form of snow and *vice versa.*

Prof. Turner. There is an acknowledged annual term known as the Kimura effect, but even that, as I understand, has not revealed itself to the analysis of the authors.

Prof. Larmor. I was just reading about that in the last published report by Albrecht, 1906. A change of latitude common to all longitudes must mean change in the Earth's figure of magnitude, such as would be appalling. It is reasonable, then, to think it must be some instrumental effect, and not a real change of latitude. To reduce the observations it is necessary to throw out this constant term, in order to make everything go as it ought for a circle of observatories round the Pole. It is fortunate for this point of view that it is mainly annual, because that at once suggests climate, and the effect of climate on the instruments is potent. It seems, moreover, to be smaller than was thought at first.

Dr. Lockyer. One variation, not mentioned before, which might partly account for the origin of the movement of the Pole, is the different changes in the meteorological elements from year to year. For instance, some recent work has shown that there is very considerable transfer of air from the Western Hemisphere to the Eastern Hemisphere, and *vice versa.* Taken over such large areas, this matter must amount to a very considerable quantity and must have an effect on the movement of the axis of the Earth. I might mention that this barometric oscillation covers roughly a period a little over three years, and from the four curves on the screen one notices that, beginning at 1900, there was revolution round the point of origin, which was not four years but just over three, so that there might possibly be some relation. You want a big transfer of material, and I do not think an earthquake, for instance, would give that big transfer.

Mr. Lindemann. Some years ago I conceived a similar idea and made calculations on this same basis. I collected meteorological observations made in Siberia and in Russia and compared them with those of Australia, and found the difference of pressure and temperature something enormous.

Prof. Larmor. I quite agree with Dr. Lockyer's point that displacement of material over the Earth is due partly to barometric causes and partly to the melting of polar snow, and perhaps oceanic change of level. In any case discussion on such matters will, I think, be much helped by the curves which Major Hills has drawn. Of course in the beginning of the investigation the only thing that could be done to make a theoreticäl view of the Polar motion at all was to asssume free precession and then to assume an annual (meteorological) period on the top of it. The object of this graphical way of treating it is that it avoids assuming any periodicity on the top of the free precession. We find out a graphical trace of what there is, instead of assuming an annual period and trying to fit it in. Such an annual part is only a small part of the total force-system shown in the diagrams; and that is the end of the matter at present.

The President. We must all appreciate the chief point of this paper, although we have not yet discussed all the details, which we look forward to reading in the 'Monthly Notices.' I think this is another case in which the discussion might be reopened at a future date by the aid of a supplementary paper.

Prof. Whittaker then read two papers, entitled respectively "On the Distribution of Energy in the Continuous Spectrum" and "On the Resolving Power of Spectroscopes." He said that perhaps some apology was needed for offering papers on theoretical spectroscopy to the Royal Astronomical Society, but the matters treated of in the papers had some astronomical applications. The first paper consisted of a theoretical justification for a law which had been inferred from observation, namely, that the intensity in the continuous spectrum is, for the longer wave-lengths, proportional to $\lambda^{-4}T$, where λ is the wave-length and T the absolute temperature. Various attempts had been made during the last few years by different scientists to prove this law from the theoretical side, by the help of assumptions regarding the mechanism of radiation, *e. g.*, assuming that the radiating body contains a great many electrical vibrators, each having its own period of free vibration and exchanging energy with the æther as well as with the material molecules. In the present paper it was shown that the law could be explained without making any assumptions regarding the mechanism of radiation, and that, in fact, it was a necessary consequence of the ideas now generally held regarding the nature of white light. White light was now considered to consist of a succession of "pulses," and the essence of the paper consisted in showing that when such a pulse, consisting of compensating positive and negative parts, was resolved into its constituent waves, the energy contained between wave-lengths λ and $\lambda+d\lambda$ was proportional to $\lambda^{-4}d\lambda$, for values of λ large compared with the length of the pulse. When this was proved, which was a matter of pure mathematics, the law in question could be at once derived.

In the second paper it was shown that for all possible types of spectroscope the resolving power, as defined by Lord Rayleigh, is equal to the number of alternations of phase in the disturbance which is produced from a single incident pulse.

Prof. Larmor. I do not know but that Prof. Whittaker's demonstration does not come near proving a physical fact by pure mathematics. I would like to have time to think what physical basis is involved. The question of the degree of statistical regularity that exists in actual radiation is hardly quite settled. Lord Rayleigh has expressed misgivings as to the amount that I have claimed in a recent paper. Anyhow, Prof. Whittaker's paper has introduced some very complex and difficult matters which are fundamental for physical astronomy, and so well worthy of the sustained attention of the Society *.

Prof. A. Fowler. I hoped Prof. Whittaker would have had time to tell us a little more about resolving power, because for some time past I have interested myself in observing the practical resolving power of certain instruments, and I have formed a strong opinion that the purity realized is a good deal higher than it should be according to the formulæ given by Schuster and others. I think I am right in understanding that Prof. Whittaker's present work does not modify the formula for the theoretical resolution power.

The following papers were announced and partly read :—

H. C. Plummer. "On the Effects of Radiation on the Motion of Comets. Second Note."

E. Nevill. "On the Early Eclipses."

E. Nevill. "The Early Eclipses of the Sun and Moon."

H. C. Plummer. "Note on a Mechanical Solution of Kepler's Equation."

H. H. Turner. "On the Possibility of Improving the Places of the Reference Stars for the Astrographic Catalogue from the Photographic Measures."

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* [I take advantage of the opportunity afforded by proof-correction to assent to Prof. Whittaker's position, which is that the law of intensity of actual radiation for long waves, viz. $\lambda^{-4}T$, where T is the absolute temperature, rests upon the thermodynamics of Bartoli, Boltzmann, and Wien, combined with Fourier's theorem, and involves no further hypothesis or statistics as to the nature of radiation, the restriction being that the formula applies only to wave-lengths that are long compared with the independent sequences or pulses that in their aggregate constitute the radiation. Even in a layer of radiating gas the continuous background in the spectrum which arises from pulses excited by collisions must ultimately range up with the regular selective radiations of the lines of its spectrum when the thickness of the layer is great enough.—J. L.]

A. S. Eddington. "The Systematic Motions of the Stars."

H. N. Russell. "Stellar Parallax Papers.—No. 3. The Parallax of Eight Stars, from Photographs taken at the Cambridge Observatory."

Rev. A. C. Henderson. "Auroræ observed at Delting, Shetland, 1905–6."

A. R. Hinks. "Solar Parallax Papers.—No. 5. Examination of the Photographic Places of Stars published in the Paris Eros Circular."

Royal Observatory, Greenwich. "Observations of the Satellite of Neptune from Photographs taken between 1905 December 19 and 1906 April 25."

W. E. Plummer. "Cometary Observations at the Liverpool Observatory."

P. H. Cowell. "The Mediæval Eclipses of Celoria."

Max Wolf. "Extended Nebula near 26 Ceti."

E. T. Whittaker. "On the Distribution of Energy in the Continuous Spectrum."

E. T. Whittaker. "On the Resolving-power of Spectroscopes."

Prof. J. Larmor and Major Hills. "The Irregular Movement of the Earth's Axis of Rotation; a Contribution towards the Analysis of its Causes."

The following gentlemen were elected Fellows of the Society:—

Ralph Falcon, M.A. Oxon., Camerton Hall, Workington, Cumberland.

Arthur Grant Stillhamer, Yerkes Observatory, Williams Bay, Wisconsin, U.S.A.

The following Candidates were proposed for election as Fellows of the Society:—

Rodney Boyce, Soudan Survey Department, c/o the Royal Colonial Institute, Northumberland Avenue, S.W. (proposed by E. P. Cotton).

Arthur Cleminson, Deputy Commissioner of Lands, Lagos, West Africa (proposed by E. P. Cotton).

George Innes, M.P.S., Chemist and Optician, Olive Bank, Edinburgh (proposed by Rev. John Spence).

Arthur Kent Lucke, Suez Canal Company's Service, Transit Department, Ismaïlia, Egypt (proposed by E. W. Maunder).

George Street, M.A., Tutor, Merton House, Southwick, Sussex (proposed by Arthur R. Hinks).

ANNUAL GENERAL MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1906, October 31.

A. C. D. CROMMELIN, B.A., *President*, in the Chair.

Secretary: J. G. PETRIE.

MR. PETRIE read the Minutes of the last Annual General Meeting. The list of presents received was next read, and the thanks of the Association were accorded to the respective donors.

The names of 9 Candidates for Membership were read and passed for suspension, and the election by the Council of 2 new Members was unanimously confirmed.

Mr. Petrie then read the report of the Scrutineers of the Ballot for the Session 1906–07. The following were declared duly elected:—*President*: F. W. Levander. *Vice-Presidents*: A. C. D. Crommelin, E. W. Maunder, S. A. Saunder, W. H. Wesley. *Treasurer*: W. H. Maw. *Editor*: F. W. Levander. *Librarian*: George Bruford. *Secretaries*: J. A. Hardcastle, James G. Petrie. *Other Members of the Council*: G. J. Burns, G. F. Chambers, H. Ellis, Capt. F. L. Grant, W. Heath, H. P. Hollis, W. T. Lynn, Mrs. E. W. Maunder, H. K. Moore, C. Thwaites.

The following nomination of Members by the Council as Editor and Directors of the Observing Sections was unanimously confirmed:—*Editor*, F. W. Levander. *Sun*, Rev. A. L. Cortie; *Moon*, Walter Goodacre; *Mercury and Venus*, Henry MacEwen; *Mars*, E. M. Antoniadi; *Jupiter*, Rev. T. E. R. Phillips; *Saturn*, G. M. Seabroke; *Comets*, E. W. Maunder; *Meteors*, Miss Catharine O. Stevens; *Variable Stars*, Col. E. E. Markwick; *Double Stars*, G. M. Seabroke; *Photography*, F. W. Longbottom.

The adoption of the Report of the Council on the work of the Session, October 1, 1905 to September 30, 1906, together with the accounts, was carried unanimously, as were votes of thanks to the retiring Members of the Council, the Auditors, and the Scrutineers of the Ballot.

The President then delivered his address, several illustrations being thrown upon the screen with the aid of the lantern. After a brief reference to the stationary membership and to some criticisms of the Journal he said that he had dwelt at some length in his address of the previous year on the new satellites of Jupiter and Saturn, and the problems they presented. Thanks to the labours of Dr. F. E. Ross, their orbits were now accurately known, and their positions could be predicted so accurately that there would be little difficulty in keeping them permanently under observation. Mr. F. J. M. Stratton had contributed a very interesting paper to the 'Monthly Notices,' dealing with the problem of Phœbe's retrograde motion, and examining Prof. Pickering's suggestion that the rotation of Saturn was originally retrograde, and that solar tides had reversed the direction of rotation after the birth of Phœbe, but before that of the other satellites. Mr. Stratton's work had been most laborious and difficult, and his results were decidedly in favour of this suggestion.

In mentally reviewing the astronomical progress of the past year, it seemed to him that a prominent place must be given to Mr. Lewis's work on Double Stars, recently published as a Memoir of the Royal Astronomical Society. Double Stars had the great interest of extending our experimental knowledge of gravitation beyond the solar system. They, including spectroscopic binaries, afforded the only means of learning anything of the masses of the stars. They showed also the wonderful variety that prevailed in the universe. The number of binary stars for which a determination of the relative masses had been obtained was 17, of which 8 determinations were due to Mr. Lewis himself. The results were sufficiently startling: in 11 cases out of the 17 the fainter star was the more massive; in three more the masses were equal, leaving only three in which the brighter star was the more massive, and even in those cases the disparity in mass was very much less than in brightness. Mr. Lewis had also pushed his investigations in another direction and by dividing the doubles into fixed and those showing relative motion, and plotting the ratio of the first to the second for the different regions of the heavens, had arrived at results which suggested that the disparity was due to the shape of the universe of stars around us. It would certainly appear that Mr. Lewis had hit on a new and powerful method of gauging the star-depths. The President then discussed at some length the cases of two difficult binaries, δ Equulei and 61 Cygni.

Dr. Downing in proposing a vote of thanks to the retiring President for his conduct during his term of office, and for the address which he had just delivered, said that during Mr. Crommelin's term of office the high traditions of the office had been fully maintained. No point that had arisen had been too intricate for him to elucidate; and, what was perhaps more difficult to a professional astronomer, no point had been too simple for him to explain. His conduct of the Meetings, too, during his term of office had been admirable. Mr. Crommelin had also earned their thanks for the admirable address he had given, elucidating as it did various points of interest that had cropped up in astronomy during the year.

Mr. S. A. Saunder, in seconding the vote of thanks, said that during the two years in which Mr. Crommelin had occupied the Chair the subjects which the Association had discussed had ranged over practically every branch of astronomy, and at the conclusion of practically every paper they had always looked to the President for some very valuable and illuminating remarks on the subject without his ever once failing them.

The motion was carried unanimously.

The President briefly thanked the Members for the warm vote of thanks accorded to him.

At the request of the President, *Mr. W. Goodacre* gave the substance of a paper communicated by *Prof. W. H. Pickering* dealing with Mr. Tomkins's paper on the bright streaks on the Moon, and also Mr. Tomkins's reply. At the same time Mr. Goodacre showed some lantern-slides illustrating the streaks on the Moon.

Mr. Goodacre then read notes on the recent lunar eclipse, observed in Australia and New Zealand, by *Mr. W. F. Gale*, *Mr. J. T. Ward*, and *Mr. W. B. Harris* respectively.

Mr. Saunder, speaking with reference to the question of the rays, said Mr. Tomkins seemed to treat them as if they were mere surface phenomena, whereas almost all the points connected with them seemed to indicate that they must be due to some deep-seated cause.

The President said he found a difficulty in accepting some of Prof. Pickering's conclusions about the presence of hoar frost, vapour, and so on. If there were frost and vapour on the Moon they would expect to see some effects of refraction in occultation. Mr. Saunder had shown very conclusively that the bright streaks could not be mere surface streaks, but must have a deep-seated origin. In particular, there seemed no way of explaining the long ray from Tycho, which stretched right across the disc, except by the idea of there being some kind of crack in the globe.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

At the Meeting of the Royal Meteorological Society on Wednesday, November 21, *Mr. W. Marriott* read a paper on "The Abnormal Weather of the Past Summer and some of its Effects."

The principal features of the weather over the greater part of England—especially the south-east—were the high state of the barometer throughout the whole of the period, except a portion of August; the high temperature in July, August, and September; the great amount of sunshine; and the deficiency of rainfall. Over the south-eastern portion of England more than 900 hours of bright sunshine were recorded during the four months June to September; while at a few stations in the extreme south and on the east coast over 1000 hours were recorded. The sunshine was more than 200 hours above the average over the Thames basin and on the coasts of Lancashire and North Wales.

The most remarkable feature of the weather during the past summer was the exceptional heat-wave which occurred between August 30 and September 3. The temperature rose above 90°

over a large part of England on four consecutive days, viz.: August 31 to September 3. Mr. Marriott has not been able to find any previous record of readings over 90° for a similar period. The air was very clear, and brilliant sunshine prevailed over nearly the whole of the country. Another remarkable feature connected with this heat-wave was the great dryness of the air; for on September 1 and 2 differences of 25° were observed between the readings of the dry and wet bulb thermometers, and relative humidities below 30 per cent. were recorded at many inland places. Owing to the great heat vegetable matter became very inflammable, and consequently there were more stack-fires than usual, and extensive stretches of heather and gorse were also set on fire.

The author said that with the advent of the hot weather the death-rate increased considerably; and he pointed out that when the mean maximum temperature for the week reached 72° the death-rate at once began to rise. The increase of the death-rate was made up almost entirely of infants under one year of age. This was shown to be due to the prevalence of infantile epidemic diarrhœa, which sets in when the mean maximum temperature for the week rises above 72°.

Attention was called to the effect which the high temperature had in turning milk sour and in rendering it unfit for drinking purposes, unless it had been first Pasteurized or sterilized. Not only was the ordinary milk a source of danger to infants during the hot weather, but the great use which is now made of tinned foods also tended to produce ptomaine poisoning and cause diarrhœa.

'Owing to the drought, keeping for cattle was very deficient, and consequently there was a falling off in the milk-supply of as much as 30 per cent.

The brilliant sunshine and the warm weather had a great effect upon holiday traffic, for more visitors than usual went to the various seaside and holiday resorts, and the outdoor entertainments were successfully carried on without any interruption by rain.

It is evident, from what Mr. Marriott said in his paper, that the weather of the past summer was of a very exceptional character, and whatever its deleterious effects may have been, we in England can fully endorse the sentiment expressed in 'Punch' bidding farewell to the summer, and wish it "Au revoir!"

Dr. H. R. Mill gave an account of the proceedings at the International Congress on Polar Exploration held at Brussels in September last, which he attended as the delegate from the Royal Meteorological Society.

On an Astronomical Point in Dante's 'Purgatorio.'

THERE is a subtle point connected with Dante's position as regards sunlight and shadow on the third day that he spends on Mount Purgatory that I have reason to believe has not yet been alluded to in print. Mr. Wicksteed, whose knowledge of Dante is like Dante's knowledge of Virgil, explained it to me in conversation, and I calculated some azimuths of the Sun for him in connection with it. By his permission I write this paper in the *Observatory*, for, among all readers of Dante, the point is one that appeals especially to astronomers.

Mount Purgatory is a very lofty conical mountain at the antipodes of Jerusalem. Its upper part is girdled by seven terraces, one of which is eighteen feet wide, each terrace being allotted to the purgation of a particular sin. Dante is visiting the mountain at Easter, when the days are twelve hours in length, the Sun rising in the east, culminating in the north and setting in the west. Dante always goes along the terraces with his right hand outside in the same direction as the Sun.

During the second night Dante sleeps on the fourth circle, and towards morning he dreams of the Siren, which typifies excessive love for things good in moderation. The corresponding vices—avarice, gluttony, and incontinence—are punished in the fifth, sixth, and seventh circles that he is going to visit that day.

He wakes up when it is high day, and he and his guide Virgil walk with the new Sun at their backs. They are therefore at the north point of the fourth circle and are walking westward. They immediately mount to the fifth circle, where they found Pope Adrian V., who for little more than one month felt the weight of the papal mantle, and found that the height of earthly ambition did not bring peace; and also Hugh Capet, who denounces the crimes of his descendants, the Kings of France, committed "to make amends," and culminating in the captivity of Pope Boniface VIII. ("nel vicario suo Cristo esser catto"). The mountain then shakes, as the soul of Statius completes its purgation. Statius joins the two other poets, and Virgil (this is the point of the present paper) tells Statius that Dante is alive. Statius expresses his reverence for Virgil, and Dante tells him that his companion is Virgil. Statius attempts to embrace him, forgetting his nothingness and treating shades as a solid thing ("quando dismento nostra vanitate, trattando l'ombre come cosa salda"). The three poets go on together, talking of Statius' secret conversion through Virgil's fourth eclogue, and they mount to the sixth circle. By this time it is past ten, for already four handmaids of the day were left behind and the fifth was at the chariot pole. They meet Forese Donati, a Florentine friend of Dante's, who exclaims as he recognises Dante, "What grace is this to me?" They talk for some time, Forese denouncing the women of Florence, and then at length Forese speaks of Dante's shadow, whereby he and others know that Dante is a living man.

Dante next meets Bonagiunta and they talk of Dante's poetry, and Forese predicts Corso Donati's death. At length as they leave the sixth circle to mount to the seventh it is two o'clock, for the Sun has left the meridian to Taurus.

Incontinence is punished by fire in the seventh circle, and Dante's shadow (it is now nearly sunset) makes the flames more red: and the spirits within note thereby that he is alive. Incidentally the reader notes that Dante has come to the west of the mountain, for only so would his shadow fall on the flames, or rather on that part of the flames that are nearest to him. Dante speaks with Guido Guinicelli and Arnaut Daniel. The time is then so near sunset at Purgatory that the first rays of the new day are visible at Jerusalem. Dante passes through the flames and mounts eastwards with his shadow in front of him. After a few steps the Sun sets and further progress is impossible.

Now this is Mr. Wicksteed's point. Why did Virgil tell Statius that Dante was alive? If Dante was in sunlight, Statius would have seen this for himself, as other spirits did. Moreover, Dante starts in sunlight, and while the Sun travels 180° from east to west, Dante only travels 90° from north to west. On the whole, therefore, the Sun travels twice as fast as Dante. How is it that Dante appears to have got round the corner into the shadow? And the answer is:—In low latitudes the Sun rises very steeply and moves very slowly in azimuth. Dante would be in the shadow if at the beginning of the day he travelled slightly in excess of his average pace.

Further, it is possible that by the time he meets Forese Donati he had already got nearly to the west side of the mountain. It was then past ten in the morning, but from 2 P.M. to 6 P.M. he was mounting from the sixth to the seventh circle and not moving round the mountain. In that case perhaps Dante was in shadow when Donati first addressed him, and "What grace is this to me?" merely expresses the latter's joy at seeing an old friend. Later in the same conversation Donati and others perceive Dante's shadow. Perhaps it was at this moment that the Sun came round the corner of the mountain.

P. H. COWELL.

The Sun-spots of 1906.—V.

MORE separate spot-groups were observed on the Sun in 1906 September than in the preceding month, but there was no increase in the mean daily spotted area; indeed, if anything, there was a slight falling off. The month inherited no fewer than eight groups from August, but transmitted only three, and those but small, to October. One group of September—Group 5968—was a very fine one, and interesting from its form and changes, as well as from its great extent.

Rotation 709 began September 24$^{\mathrm{d}}$·73.

Spot-Groups in 1906 September.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	d	°	°	
5958	Sept. 1	Sept. 8	8	Sept. 4·5	267	−11	Small changing stream.
9	1	1	1	5·0	261	+ 7	Very small spot.
5960	1	12	12	8·2	218	+18	**Return.** Stream.
1	5	14	10	9·7	199	−23	**Return.** Stream.
2	6	8	3	7·2	231	−10	Small spot.
3	7	11	5	6·2	244	+24	Spot. Rapid change.
4	9	10	2	12·2	166	+12	Small spot.
5	9	20	12	13·7	146	+ 9	Straight stream.
6	10	17	8	16·9	103	+ 3	Double spot.
7	12	14	3	9·5	201	+ 4	Small spot.
8	12	24	13	17·5	96	+ 7	**Return.** Fine stream.
9	15	24	10	20·9	51	−21	Long sparse stream.
5970	15	20	6	21·9	38	− 9	Small spot.
1	20	21	2	25·6	349	+20	**Return.** Small spot.
2	21	25	5	26·9	331	−14	Short irregular stream.
3	22	26	5	20·6	55	+12	Small faint spots.
4	22	23	2	21·2	47	+10	Few small spots.
5	23	29	7	23·7	14	−17	Small stream.
6	24	27	4	30·6	283	+ 6	Few small spots.
7	25	27	3	22·4	31	−11	Small spot.
8	25	26	2	25·6	349	+ 6	Very small spot.
9	25	Oct. 6	12	Oct. 1·1	276	−22	Very small spot.
5980	26	Sept. 26	1	Sept. 26·4	338	− 9	Very small spot.
1	27	27	1	25·3	352	− 1	Very small spot.
2	28	28	1	25·1	355	− 8	Very small spot.
3	28	Oct. 6	9	30·6	283	−20	Small spot.
4	28	Sept. 30	3	30·8	280	+ 4	Very small spot.
5	29	Oct. 1	3	Oct. 1·0	277	−28	Small spot.

Notes on the Principal Spot-Groups.

Group 5960.—Return of Group 5936, or possibly merely a revival. A few small irregular spots, developing later into a straight stream. Area about 100.

Group 5961.—Return of Group 5938, or possibly merely a revival. An irregular cluster of small spots, developing later into a straight stream. Area about 100.

Group 5965.—A straight stream. Area about 100.

Group 5966.—A double spot, area about 80, which steadily diminishes in size. It acts as forerunner to Group 5968.

Group 5968.—Possibly a return, but more probably a revival of Group 5942. A few small spots *n. f.* Group 5966 at first, but rapidly developing into a very fine stream, 16° in length. Area about 500.

Group 5971.—Return of Group 5946. Fifth and final apparition. Area about 5.

Groups 5979, 5983, and 5985 form parts of a wide area of feeble disturbance. E. Walter Maunder.

The Planets and Planetary Observation.

[Continued from p. 418.]

Venus.

In turning to the consideration of Venus we are at once confronted by the fact that some very similar doubts are attached to the records of the telescopic aspect and to the determination of her rotation-period as exist in the case of Mercury. There is really much uncertainty in the evidence we have accumulated both as to her markings and as to the length of her day. It is true some of the difficulties affecting successful observation of Mercury do not apply to Venus. For Venus sometimes presents a large disc, though crescented at the time, and is seen under such an angle that we might expect to discern comparatively small spots on her surface. And, moreover, this planet, under favourable circumstances, may be visible for a fairly long period after sunset or before sunrise.

Thus, during the last half of April 1892 and 1900, Venus set about 5½ hours after the Sun, and the same conditions will recur in 1908, 1916, &c. On the other hand, Mercury, at a very favourable eastern elongation, will only set about 2¼ hours after the Sun, as at the middle of May in 1883 and 1896, and again in 1909 and 1922. So that, in respect to apparent diameter and duration of visibility in the Sun's absence, Venus possesses a marked advantage, though her spots or inequalities of tone are decidedly fainter and more indefinite than those of Mercury. In fact there are some observers, whose opinions are entitled to the highest respect, who have insisted on the practically featureless aspect of Venus.

Like Mercury, Venus may be best observed at the evening apparitions which occur in the first half of the year, and at the morning apparitions in the last half, because at such periods the position is N. of the Sun's place, and she is a long time above the horizon in the absence of that luminary. Her elongations usually extend to between 45½° and 47°, and she displays maximum brilliancy at a time preceding or following inferior conjunction by

36 days, when her disc is one-fourth illuminated and her apparent diameter about 40″.

Sir W. Herschel could detect nothing of Schröter's alleged mountains on Venus, and was led to assert that "no eye which is not considerably better than mine, or assisted by much better instruments, will ever get a sight of them." Though Herschel examined the planet a good deal from 1777 to 1793 with a variety of telescopes, he could obtain no definite and convincing evidence as to the period and direction of the orbital motion of Venus. He concluded that the atmosphere of the planet was of greater extent and refractive power than that given by Schröter, and described her as being "suddenly much brighter all around the limb." Herschel sometimes perceived spots on the crescent of Venus, and they appeared to support a rotatory motion of the planet much more rapid than Bianchini had supposed, but they were too faint, indistinct, and variable to afford any exact determinations.

Sir John Herschel thought Venus a very difficult planetary object. "The intense lustre of her illuminated part dazzles the sight and exaggerates every imperfection of the telescope *; yet we see clearly that her surface is not mottled over with permanent spots like the Moon, we notice neither mountains nor shadows, but a uniform brightness on which sometimes we may indeed fancy, or perhaps more than fancy, brighter or obscurer portions, but can seldom or never rest fully satisfied of the fact." His opinion was that "we do not see, as in the Moon, the real surface of these planets (Mercury and Venus), but only their atmospheres much loaded with clouds."

Galileo appears to have been the first telescopic observer of Venus, and he discovered the phases of the planet on 1610 September 10, but for some years no further advances appear to have been made.

In 1643 Francesco Fontana, at Naples, saw irregularities along the inner edge of the crescent. In 1645 he detected a dark spot. Fontana was one of the earliest and more successful of planetary observers, for he also appears to have been the first to discover spots on Mars (1636). He observed the belts on Jupiter in the same year (1630) that they were discovered by Zucchi and Bartoli, and was the first to witness a transit of Mercury (1645).

In 1665 Burrattini is said to have discerned spots on Venus similar to those on the Moon.

As Tycho Brahe was the greatest observer of the sixteenth century, and William Herschel of the eighteenth, so was J. D. Cassini the leader in the seventeenth, and he it was who, at Bologna, attentively studied, if he was not the first to discern, markings on Venus. His earliest observation † of them was on

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* When, however, the air is good and observations are conducted in daylight, this object is often sharply defined and without glare, and the study of her surface can be as effectively pursued as in the case of any other planet.

† Many interesting details of old observations of Venus are quoted in Breen's 'Planetary Worlds' (1854).

the evening of 1666 October 14, when he saw a bright point near the inner edge, and removed considerably from the centre of the disc towards the N., and also two obscure spots with a third more extensive at the W. part, but they were of the last degree of faintness. On 1667 April 20 he detected a bright spot on the S. hemisphere, and an elongated dusky spot at the E. margin and nearer the N. horn. He watched the markings long enough to perceive changes in their position, and continued to scrutinize the planet in May and June of the same year, and concluded that the axis was greatly inclined to the plane of the orbit, and that the period of rotation was 23^h 21^m, though the exact time was somewhat doubtful. He was not able to see any spots on Venus from the Paris Observatory, and regarded the air as insufficiently good for such delicate observations.

Sixty years later Bianchini, in the clear Italian sky, and using one of Campani's telescopes of 2½ inches aperture and 66 feet focal length, recovered the spots, but thought they moved very slowly in periods of 24⅓ days. On 1726 February 9 he saw two dark spots extending inwards from the terminator, both of a rounded shape, a large one near the S. horn and a small one towards the N. horn. On February 14 the large spot had vanished, but he recognized the small one towards the S. horn, and followed by two new ones connected with it. He noticed a different series of spots in 1727 July, August, and September, and in 1728 January, always deducing the same diurnal rotation from them. This observer states that "on one occasion (1726 February 26) he was interrupted for 3 hours, and that at the end of that time he felt quite certain the spots were only 2° removed from the place they were in before." Bianchini confidently relied upon his results, and prepared a map displaying the surface configuration of the planet. He called the dark regions the Seas of Marco Polo, Columbus, Vespucci, and Galileo, and other formations by various titles, such as the Promontory of the Academy of Sciences, &c. Cassini had observed that the displacement of the spots was from north to south, and considered this as a libratory movement. Bianchini saw them travelling in a contrary direction, but Breen explains that this is a necessary consequence of the different position of the axis to the observers, the N. pole in 1726 being exposed to the Sun, and in that part of Venus hidden from us, whilst in 1667 it was out of the hemisphere exposed to the Sun and in the side turned away.

In 1700 La Hire had, with an old refractor of 16 feet focus, power 90, perceived irregularities which could have been produced by mountains higher than those of the Moon.

Derham also says (1715): "Upon observing Venus with the glasses of Mr. Huyghens (98 feet focus), I thought I perceived sinuosities and inequalities on the concave part of the illuminated margin, resembling those which we see on the New Moon" ('Astro-Theology'). Schröter also remarked the toothed or serrated appearance of the terminator and it was seen by Chladni Olbers and others.

In 1740 the younger Cassini, at Paris, essayed to discern the markings on Venus which had been delineated by Bianchini, but was unsuccessful, though he used two telescopes, one of Campini's of 82 feet focal length, and another by Hartsoeker of 114 feet focus. The climate of Paris seems to have been unsuitable for the work, for Maraldi, one of the best observers of his day, also failed to see the spots at the same place. James Cassini, however, reconciled the discordant periods of rotation determined by his father and Bianchini, showing that the period of 24⅓ days given by the latter really included 25 rotations of the planet.

In 1749-50 January 23, the Rev. Dr. Miles, of Tooting, and some friends, observing Venus with one of Short's reflectors, greatest power 200, saw the planet with great distinctness through a red auroral cloud rising up from the S.W. They all plainly saw the spots resembling those of the Moon. In 1788 Bode distinguished some dark spots.

For a long time—about 9 years—the diligent Schröter, at Lilienthal, examined Venus unsuccessfully, but was at length rewarded, in 1788 February and March, by detecting some faint dusky streaks which he compared with the obscure markings seen by J. D. Cassini. On February 28 he remarked a grey, nebulous, ill-defined and excessively faint spot, and on March 3 noticed a similar appearance, but it had moved from the right-hand margin to the middle of the enlightened part of the disc. On March 5 he distinguished the markings again, but they were of such a character as to lead him to confess frankly that he felt inclined at times to regard them as optical illusions, but could not quite persuade himself that this was so. Pursuing his studies of the planet he noted other spots and traced alterations in their positions, which led him to adopt a rotation-period in very close agreement with that of Cassini. Apart from feeble dusky spots, Schröter noted that the extremities of the crescent were occasionally truncated. On 1789 December 28, 1790 January 30, and 1793 February 27, he remarked, near the S. horn, a luminous point or cusp entirely isolated, that is to say, separated by an obscure patch from the rest of the crescent. He inferred this to represent a mountain-peak, and computed the height as 145,200 feet, or 27½ miles. From this and similar appearances he obtained a rotation-period according with that derived from the shadings.

In 1796 July 7-13 Flaugergues (discoverer of the great comet of 1811) saw a bright spot on Venus in the form of a rounded trapezoid, and inferred, from the motion, rotation in about 24 hours.

In 1813 Gruithuisen remarked some long vertical shadings and some brilliant round specks.

In 1833 and 1836 Mädler observed Venus and discerned a number of feeble spots, but they were too dim to exhibit motion.

His results supported Cassini's period, and were inconsistent with Bianchini's. He saw the terminator very unevenly curved, similarly to La Hire in 1700, and attributed a very rugged condition of surface to the planet.

In 1836 Lamont, at Munich, failed to discern spots with an 11-inch O.G.

In 1839–41 De Vico and assistants, at Rome, with the 6¼-inch Cauchoix O.G., were very successful. No less than 145 drawings were made of the faint dusky clouds visible on the planet. The motion of these features conformed with the rotation-periods of Cassini and Schröter. Of six observers the most successful were those who experienced the greatest difficulty in detecting minute companions of bright stars. Webb explains this on the view that a "very sensitive eye which would distinguish the spots more readily would be easily overpowered by the light of a brilliant star so as to miss a very minute one in its neighbourhood." This explanation no doubt applies if the observations of Venus were conducted in the daytime, but if they were made after dark Venus is then such a brilliantly glaring object in a telescope that the faint shadings would be obliterated to sensitive eyes.

In 1844 April Schumacher detected a dusky spot in the twilight. Other markings which were visible in a small telescope he found effaced in a larger one.

In 1853 December and 1854 January, James Breen, with the Northumberland telescope at Cambridge, noticed the almost continual bluntness of the S. horn in contrast with the pointed and projecting N. horn. He perceived decided traces of spots, but they were too indefinite to exhibit motion.

[To be continued.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Brightness of Mercury.

Gentlemen,—

In an interesting letter on "The Brightness of Mercury," published in your issue of this month, Mr. Whitmell asks for information as to how to estimate the variation in the reflective power of a planet due to (*a*) the relative position of the incident and reflected rays, (*b*) difference of albedo for different points of the surface.

I assume (1) that the albedo of the planet is uniform over the surface, (2) that reflection by the surface is diffuse, *i. e.* the illuminated planet sends light equally in all directions.

Let S be the centre of the Sun, P the centre of the planet, M a point revolving round P, SP = R, PM = r, radius of planet = a, angle SPM $=\theta$. With P as centre, construct a sphere of radius a, and draw planes APA′ and BPB′ through P perpendicular to SP and MP respectively. Then light received from Sun by that portion of the planet visible from M varies:—

(1) Inversely as the square of SP $\left(=\frac{1}{R^2}\right)$;

(2) Directly as the projection on the plane AA′ of the lighted surface visible from M $\left\{=\frac{\pi a^2}{2}(1+\cos\theta)\right\}$;

and light received by M from the planet varies:—

(3) Directly as the albedo (A);

(4) Inversely as the square of PM $\left(=\frac{1}{r^2}\right)$;

(5) Directly as the projection on BB′ of the lighted surface visible from M $\left\{=\frac{\pi a^2}{2}(1+\cos\theta)\right\}$.

The whole light received at M from the planet P therefore varies as $\frac{A}{R^2 r^2}\cdot\frac{\pi^2 a^4}{4}(1+\cos\theta)^2$. $\frac{\pi a^2}{2}(1+\cos\theta)$ is the illuminated portion of disc. It follows that the light of Half Moon is only one-quarter that of Full Moon.

If the albedo vary from point to point it will be necessary to calculate the projections (2) and (5) separately for the area affected by each albedo and to sum the quantities of light so obtained.

I have worked out the brightness of Mercury for the two elongations given by Mr. Denning on p. 356 with the result that from February 24 to March 6 (E. elongation being on March 1^{d} 14^{h}) the brightness diminishes nearly uniformly from 451 to 118, and that from August 8 to 18 (W. elongation being on August 12^{d} 15^{h}) the brightness increases from 79 to 366.

It would be interesting to know how far the results of this formula are verified by observation.

Town Engineer's Office, Pretoria,
1906, Oct. 29.

Yours faithfully,
H. D. Badcock.

By the courtesy of the Editors I have been allowed to see Mr. Badcock's interesting letter, and beg to send a few comments.

Of the sunlit hemispherical surface of P, the curved area of that portion visible from M certainly receives just as much sunlight as would be received by the projection of this particular area on to the plane AA′ by lines from S perpendicular to AA′.

If for the curved area we may substitute this plane projection, we shall obtain for the light received by M from this projected area the final expression given by Mr. Badcock.

But have we experimental proof that the light actually reflected to M from the curved area of P will be exactly equivalent to the light that would be reflected from the projection of this area on the plane AA′?

If the curved area were self-luminous the equivalence would hold good, but I do not think we can assume that it holds good for a surface sending out light by diffuse reflection.

As Mr. Badcock does not say whether he has consulted Dr. Spitta's paper, referred to in my former letter (p. 390), I will give a very brief abstract of some of his results.

A white unpolished sphere of plaster of Paris was exposed to parallel rays of light, so that half of it was illuminated. Let S be the source of light, P the centre of the sphere, and let the point *p*, where the line SP meets the surface, be called the pole of the illuminated hemisphere. The observer was so placed as to see the whole of this illuminated hemisphere.

By means of a wedge photometer, the luminosities of several small surface areas (the projections of which on a plane through P, perpendicular to SP, were all equal) were carefully compared with each other.

From the pole *p*, to about "latitude" 60°, the albedo was uniform and equal to 0·75. Beyond this it fell off; and at latitudes 51°, 41°, 34°, 12°, the albedoes were 0·53, 0·45, 0·28, 0·09, respectively. Of course, with a disc the albedo would have been uniform and equal to 0·75, so that a disc of the same radius as a hemisphere reflects more light. Mr. Proctor states that the ratio is as 3 to 2. From Dr. Spitta's experiments, it appears that for equal projected areas of a uniform surface the albedo varies considerably with the slope.

I must not close without thanking Mr. Badcock for his communication.

Leeds, 1906, Nov. 24. C. T. WHITMELL.

The Telescopic Duplicity of Mizar.

GENTLEMEN,—

I am greatly obliged to Miss Clerke for referring me to the place in Riccioli (which is also mentioned in Grant's 'History of Physical Astronomy,' p. 558) where he speaks of the telescopic duplicity of Mizar. It is in the 'Almagestum Novum,' vol. i. p. 422, and is not referred to in the Index under Ursa Major (whence my difficulty in finding it), but placed in a chapter headed "De Apparenti et Vera Magnitudine Fixorum." Riccioli does not mention when he first noticed the duplicity, but his language would lead to the impression that it was known before him. He says that the way to determine the diameters of the stars is by comparing them with that of the Moon or (better, on account of her brightness) with the distances of neighbouring stars; also by noticing how many are required to fill up a known space in the sky, care being necessary in the estimation to allow for the effects of adventitious light increasing the size of the stars and so diminishing the apparent intervals between them; thus, he adds, the star in the middle of the tail of Ursa Major (he seems persistently to ignore Bayer's nomenclature, though he must have known of it) appears to be single, though there really are two "ut Telescopium prodidit." Had it not been for this last clause I should have thought he was referring to Alcor, as in p. 403, which I quoted before. The 'Almagestum' appeared in 1651, so the perception of the duplicity of Mizar must have been before that year.

Yours faithfully,

Blackheath, 1906, Oct. 5. W. T. LYNN.

Bruce and the Lunar Eclipse of April 17, 1772.

GENTLEMEN,—

In Prof. Turner's interesting notes on the eclipse expedition to Aswan (1905 August 30), which are reprinted from vol. xxviii. of the *Observatory* as no. 117 of the smaller publications of the University Observatory, Oxford, reference is made to a lunar eclipse observed by James Bruce, the great Abyssinian traveller, on the 17th of April, 1772. Perhaps it is a slip of the memory, or perhaps of the pen, but in Prof. Turner's paper the date appears as April 11; it should be April 17, which was a Friday, and, indeed, Good Friday. But the curious point about it is that Bruce says that he warned the people at Teawa when the Moon rose that she would shortly be hidden, but the obscuration would pass off, as it did. As a matter of fact, the Moon rose in Abyssinia *whilst* totally eclipsed, and it is supposed that Bruce wrote his account long afterwards, and had forgotten the exact circumstances. He tells us that he had consulted "the ephemerides," and knew the exact time of the phenomenon. It is somewhat remarkable that several of the popular almanacs of that year, viz. those of Andrews ('Remarkable News from the Stars'), Moore ('Vox Stellarum'), Partridge ('Merlinus Liberatus'), Saunders ('The English Apollo'), gave the date of the eclipse as Thursday, the 16th of April*. Bruce, however, must have had Nautical Almanacs with him on his journey, because he gives several determinations of longitude by eclipses of Jupiter's satellites, which I find occurred on the dates stated by him. Now in that for 1772, Maskelyne gives for the lunar eclipse of April 17, middle of eclipse at Greenwich $4^h\ 5^m$, end of totality $4^h\ 55^m$. As Teawa is about 35° or $2^h\ 20^m$ east of Greenwich, the totality would be over there about $7^h\ 15^m$, very soon after moonrise. This is, of course, inconsistent with the circumstances described by Bruce, but I should hardly draw so sweeping a conclusion from this as Prof. Turner does:—

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* Oddly enough, Pearse's 'Cœlestial Diary' puts it on Thursday, the 17th day, wrong in the day of the week.

"We then have to contemplate the fact that a man who was keenly interested in being accurate, ... a man, moreover, who had consulted ephemerides and knew that they could be used to check his assertions, such a man could deliberately make this erroneous record about an eclipse which he had actually seen upon a stirring occasion. Is it to be wondered at that the accounts of ancient eclipses, when there were no such incentives to accuracy, are worth so little?"

I submit that this is confusing all accounts of such phenomena too much. Human nature is much the same in all ages, and where accounts, either ancient or modern, are written down long after their occurrence, details are often erroneous. Bruce does not draw any deduction from the observation of this eclipse; he simply tells us how he astonished the natives by his prediction of it. Now if he had said that when the Moon rose she would have only a sickle of light as when a day or two old, which would rapidly increase until she appeared as round as the night before, that would have astonished them as much as the erroneous statement which he says he did make, but his memory, writing long afterwards, failed in the detail. It is also worth notice that on the 11th of the following October, when he was at Chendi, another eclipse of the Moon occurred, the middle of which took place at $5^{h}\ 27^{m}$ at Greenwich and $7^{h}\ 41^{m}$ at Chendi (longitude $33^{\circ}\ 30'$), on a night when she rose about 5 o'clock in the evening. May Bruce, after a long interval, have confused in his mind these two eclipses? Of course he kept notes during his journey, but these were lost and only recovered after a long interval of time, so that part may have been lost altogether. His book was not published until 1790, but the principal details of his travels were known, and he was much disappointed that they did not make the sensation he expected, still more that the accuracy of his accounts was called in question by many. Amongst the sceptics on the point is said to have been Dr. Johnson, who was in his company in 1775, and whose remark about him (Birkbeck Hill's edition of Boswell, vol. ii. p. 333) was:—"Why, Sir, he is not a distinct relater, and I should say neither abounding nor deficient in sense. I did not perceive any superiority of understanding." It is, perhaps, a little remarkable that in Bruce's narrative of previous travels in Abyssinia he omits mention of Lobo, a translation of whose work (with some liberty of abridgment) was the first publication of Johnson. Bruce certainly acted unfairly in contesting what seems to have been the fact, that Paez, the Jesuit, had visited the source of the Blue Nile before him.

Yours faithfully,

Blackheath, 1906, Nov. 5. W. T. Lynn.

OBSERVATORIES.

[Continued from p. 427.]

Hamburg. *R. Schorr.*—The revision of the Rumker Catalogue is proceeding slowly. The 9½-inch refractor was in use for comets and variable stars and measures of the system of Saturn. Time-service. Meteorology.

Heidelberg (Astronomisches Institut). *W. Valentiner.*—The Director pays a tribute to M. Biesbroeck, who spent some time at the Observatory as volunteer observer and computer. The 12-inch, when not used for comets and small planets, was placed at his disposal, and he made a valuable series of double-star measures for the 'Struve Memoir' of the Royal Astronomical Society. The comets observed with the 12-inch are 1904 *a*, 1904 *d*, 1904 *e*, 1905 *a*, Giacobini. The 8-inch was used with a Zollner photometer for variable stars, a very fine series of estimations being secured. The following parallaxes have been deduced by Herr Jost:—

	″		″		″
109 Herculis ...	+0·020	Bradley 2792 ...	+0·077	40 Lyncis	−0·066
110 Herculis ...	+0·058	K Aurigæ	+0·050	11 Leo Min. ...	+0·067
3 Cygni	+0·070	56 Aurigæ	+0·076	20 Leo Min. ...	+0·065
16 Cygni	+0·153	Groomb. 1281...	+0·061	Bradley 1433 ...	+0·103
15 Sagitt.	+0·080	ρ Geminor.	+0·045	Groomb. 1678...	+0·021
Groomb. 3215...	+0·006	σ Geminor.	+0·052	Lal. 21185	+0·361
56 Cygni	−0·051	Lal. 15565	+0·109	55 Ursæ Maj. ...	+0·028
Groomb. 3357...	+0·069	P. VII. 321......	+0·034	Groomb. 1774...	+0·025
61 Cygni	+0·320	32 Lyncis	+0·084	Groomb. 1830...	+0·085
65 Cygni	+0·125	ρ_2 Cancri.........	+0·087		

Heidelberg (Astrophysikalisches Institut Königstuhl). *Max Wolf.*—Celestial photographs, 698 plates exposed. Large number of observations of small planets (photographic) and 52 new planets discovered. Fourteen variable stars were observed on 78 evenings. Time-service.

Jena. *Otto Knopf.*—Comets and small planets with the 174-mm. refractor: Comets 1905 III., 1905 IV. Expedition to Algeria to observe total solar eclipse. Results very gratifying. Meteorology.

Jena. *W. Winkler.*—The 28th consecutive year's observation of sun-spots. Spots were observed in 1904 on 211, in 1905 on 270 days.

Kalocsa. *J. Fényi, S.J.*—Spectroscopic observations. Sun-spots were observed on 237 days. On two days, July 27, 28, the Sun was free from spots. Meteorology.

Kasan (Engelhardt-Sternwarte). *D. Dubiago.*—Unfavourable weather; only on 93 nights could observations be made. The 12-inch equatorial was used for observation of comets. Observations for latitude were made with the transit-circle. At the old Observatory observations of small planets with the refractor, and of Mösting A with the heliometer.

KIEL. *Harzer.*—Time-service. Meteorology.

KIEL (Astronomische Nachrichten). *H. Kreutz.*—This Observatory undertakes the publication of the *Astronomische Nachrichten*, and is also the Centralstelle für astronomische Telegramme.

KÖNIGSBERG. *H. Battermann.*—The observation of the Gill zodiacal stars was completed with the Repsold meridian-circle, and a new programme started, which includes stars between −25° and +35° Dec. belonging to the new Fundamental Catalogue of Auwers. The 13-inch refractor was in use for comets and planets. Time-service.

KREMSMÜNSTER. *P. Schwarz.*—The Director succeeded P. Wagner in January 1906. Comets and planets with the refractor. Variable stars. Time-service. Meteorology.

LEIPZIG. *H. Bruns.*—Prof. Peter continued his observations for the parallax of 61 Cygni. The triangulation of 28 stars in the Hyades has been completed and the results sent to press. A model of Saturn's System made for measuring to study systematic error. Dr. Hayn has made a new reduction of measures made by Hartwig between 1877–79 to determine the form of the Moon (*A. N.* 4083). Time-service.

MÜNCHEN. *H. Seeliger.*—With the meridian-circle 6841 observations of 1650 stars were made. Time-service. Meteorology.

NEUCHÂTEL. *L. Arndt.*—Testing chronometers and watches. Time-service. Variable stars. Meteorology.

O'GYALLA. *Anton Tass*, for the Director, *Hofrat v. Konkoly.*—The main work of this Observatory is photometry: (1) variable stars, (2) a photometric Durchmusterung of stars between 0° and 10° Dec. down to 7·5 mag. Photographs of Sun on 137 days; ten days free from spots. Meteorology. Time-service.

ROME. *E. Millosevich.*—Small planets and comets. Observation of spot, faculæ, and prominences. Geodetic work. Meteorology.

STOCKHOLM. *K. Bohlin.*—Meridian work. The astrographic refractor was employed on 101 objects with exposures varying from 15 to 60 minutes. These include nebulæ, large proper-motion stars, ζ Ursæ Majoris. The parallax of Σ 2486 is found to be 0″·064. Time-service.

STRASSBURG. *E. Becker.*—The large refractor was used by Dr. Wirtz for observations of nebulæ, comets, 266 double stars, satellites of Saturn, Uranus, Neptune, diameters of Mercury, Venus, Mars, Jupiter, Saturn, Uranus, and a number of occultations of stars by the Moon. The meridian work was by Dr. Jost.

UTRECHT. *A. Nijland.*—Small planets, meteors, variable stars (a large number).

NOTES.

Comet Notes.—Two new comets have been discovered last month. The first, 1906 *g*, was discovered by Holger Thiele at Copenhagen on Nov. 10. It was of magnitude 8·5, and the theoretical brightness went on increasing till Nov. 27, when it was 1·34 times that at discovery. On Dec. 13 it will equal the discovery brightness. It is running rapidly northward and is circumpolar in December. The following are the most accurate elements yet to hand; they are by Dr. E. Strömgren from observations on Nov. 10, 12, 14:—

T	1906 Nov. 22·39 Berlin M.T.
ω	9° 48′
☊	85 4
i	57 10
$\log q$	0·0845

Approximate Ephemeris for Berlin Midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Dec. 2....	11 21 40	41 28	Dec. 10....	12 27 6	49 56
6....	11 53 13	46 6	14....	13 3 0	53 24

The other comet, *h* 1906, is very faint (about magnitude 12). It was photographically discovered on Nov. 14 by Mr. Joel Metcalf at Taunton, Mass., U.S.A., in the course of his minor planet work. It was in R.A. $4^h\ 4^m\ 36^s$, S. Dec. 2° 16′. Two days later it was observed by Mr. Hammond at Washington ($11^h\ 38^m$ local M.T.), in R.A. $4^h\ 4^m\ 11^s{\cdot}4$, S. Dec. 2° 46′ 55″.

No orbit is yet to hand. A. C. D. C.

Minor Planet Notes.—The following new planets have been discovered at Heidelberg and Taunton, U.S.A.:—

	Date.	Discoverer.		Date.	Discoverer.
VP......	1906 Oct. 11	Kopff.	WA ...	1906 Oct. 21	Wolf.
VQ ...	11	,,	WB ...	21	,,
VR ...	11	,,	WC ...	22	Kopff.
VS......	11	,,	WD ...	13	Lohnert.
VT ...	8	Lohnert.	WE ...	26	Metcalf.
VU ...	17	Kopff.	WF ...	Nov. 9	Kopff.
VV ...	17	,,	WG ...	9	,,
VW ...	17	,,	WH ...	11	,,
VX ...	17	,,	WJ ...	11	,,
VY ...	17	,,	WK ...	11	,,
VZ......	17	Lohnert.	WL ...	11	,,

The interesting planets Thule and Andromache have also been photographed at Heidelberg.

The following planets have received permanent numbers:—QX 570, QZ 571, RB 572, RC 573, RD 574, RE 575, RF 576, RH 577, RZ 578, SD 579, SE 580, SH 581, SO 582, SP 583, SY 584, TA 585, TC 586, TF 587, TG 588, TM 589, TO 590, TP 591, TS 592, TT 593, TW 594, TZ 595, UA 596, UB 597, UC 598, UJ 599, UM 600, UN 601.

The following identities have been established:—QY 167, RA 429, RK 509, RT 477, SF 488, SK 275, SQ 411, SX 88, UD 181, UE 410, UF 394, UG 149, UH 431.

The following do not receive numbers, owing to insufficient observation:—RG, RJ, RL, RM, RN, RO, RP, RQ, RR, RS, RU, RV, RW, RX, RY, SA, SB, SC, SD*, SG, SJ, SL, SM, SN, SR, SS, ST, SU, SV, SW, SZ, TB, TD, TE, TH, TJ, TK, TL, TN, TQ, TR, TU, TV, TX, TY, TY*, UK, UL, UO, UP, UQ, UR, US.

The following have received names:—459 Signe, 461 Saskia, 464 Megaira, 465 Alekto, 466 Tisiphone, 467 Laura, 468 Lina, 469 Argentina, 471 Papagena, 473 Nolli, 474 Prudentia, 479 Caprera, 490 Veritas, 492 Gismonda, 495 Eulalia, 500 Selinur, 501 Urhixidur, 502 Sigune, 513 Centesima, 514 Armida, 515 Athalia, 524 Fidelio, 525 Adelaide, 527 Euryanthe, 528 Rezia, 529 Preciosa, 530 Turandot, 531 Zerlina, 536 Merapi, 539 Pamina, 540 Rosamunde, 541 Deborah, 549 Jessonda, 550 Senta, 551 Ortrud, 552 Sigelinde, 553 Kundry, 555 Norma, 557 Violetta, 558 Carmen, 559 Nanon, 560 Delila, 561 Ingwelde, 562 Salome, 581 Tauntonia. A. C. D. C.

EARLY OBSERVATIONS OF THE SIXTH SATELLITE OF JUPITER ('Harvard Annals,' vol. lx. no. ii.).—A search on the Harvard plates has disclosed some images of this satellite on photographs taken in 1894 and 1899. The latter images were noted by Miss Leavitt on December 10, 1904, but it was concluded that the object was a minor planet, not a satellite. The following are the dates (in G.M.T.) and the co-ordinates of the satellite referred to the centre of Jupiter:—

Date.			Satellite — Jupiter.			
			R.A.		Dec.	
		d.	m.	s.	′	″
1894	Jan.	25·563	−3	30·75	+2	51·2
	Feb.	2·489	−3	19·17	+5	41·4
1899	June	26·561	+2	54·42	+6	16·3
		27·588	+2	56·01	+6	0·7
		30·566	+2	59·30	+5	20·3
	July	1·594	+3	0·20	+5	7·4
		12·605	+2	56·38	+2	7·7

It is at once obvious that there is something wrong with one of the first two positions, for they make the apparent motion in 1894 counterclockwise, whereas it was in reality clockwise. This error must be cleared up before the 1894 positions can be used. Possibly one of the 1894 objects is not really the satellite, otherwise there is some error in the reductions. There seems to be something wrong with the reduction of the "reference star" on 1894 Jan. 25, for the measures on p. 36 show that this was 2′·0 south of the satellite, whereas the table on p. 39 makes it 0′ 31″ north of it. I have re-reduced the declination of the satellite on this day from the stars BD + 17°·527, + 16°·423, + 17°·541 and get the same figure as that given above. I think it more likely that the position for 1894 Feb. 2 is wrong. Prof. Pickering notes of this plate that the image of the satellite is excessively faint and poor and could only be found by the most careful scrutiny. I have made an attempt to deduce the sidereal period, and obtain $250^{d}\cdot2$ from the position for 1894 Jan. 25, $250^{d}\cdot3$ from that for 1899 July 12. These are subject to slight alteration when the perturbations have been computed, but are probably not seriously in error. It is incidentally noted that "the tenth satellite of Saturn, which is an exceedingly difficult object, can be seen when within 2′ of the planet," the latter being screened. A. C. D. C.

Effect of an Earthquake on Latitude.—Since it was found that the Californian earthquake of April 18 last was caused by a horizontal shearing along a geological fault, it might have been thought that the latitudes of places in the neighbourhood have undergone some sensible change. It happens that one of the International latitude stations—namely, that at Ukiah, where continuous observations for variation of latitude are being made—is but 26 miles from the fault-line, and it has been thought worth while to specially reduce the observations made before and after the earthquake. The computations show that on April 18 there was no shifting of the Earth's crust, at least of sufficient size to be differentiated from errors of observation.

Longitudes by Wireless Telegraphy.—A short account of the experiments made at Potsdam, briefly mentioned on p. 375 of the October number, has come to hand. It seems that clocks at Potsdam and the Brocken were compared by means of signals without wires emanating from a station 32 kilometres from the first place and 183 from the second, and also by means of ordinary wire telegraphy. The difference by the two methods—the experiments being made on eleven days—were never as great as the hundredth of a second.

A Bright Meteor.—A beautiful meteor was observed by Mr. Rolston at the Solar Physics Observatory, South Kensington, on Nov. $17^{d}\ 13^{h}\ 26^{m}\cdot5$. The approximate positions of the beginning and end of the track were $75\frac{1}{2}^{\circ}\ +24^{\circ}$, $-88^{\circ}\ +14^{\circ}$.

The Eclipse of 1907 January 13.—The Bureau des Longitudes are sending an expedition to Samarkand under M. Stefanik, of the

Meudon Observatory, to observe this phenomenon. Mr. Hansky, of Pulkova, in charge of a Russian expedition, will also be at the same place.

By the courtesy of Prof. Hale we have received a packet of pamphlets—Contributions from the Mount Wilson Observatory,—mostly, as would be expected, on spectroscopic matters. We hope to have an account of some of these in our next number, from the pen of Miss Agnes Clerke.

The Astronomers on the Council of the Royal Society for the coming year are Sir Wm. Huggins and Mr. H. F. Newall.

The next Meeting of the Royal Astronomical Society will be on Friday, December 14; of the British Astronomical Association on Wednesday, December 19.

On account of unavoidable delay in receiving the Ephemerides of Variable Stars, it has been found impossible to complete the Companion for 1907 for distribution with this number. We hope to publish this about December 10.

From an Oxford Note-Book.

'Lick Observatory Bulletin' No. 102 is devoted to a "Preliminary Note on some Simplifications in the Reduction of Stellar Photographs," especially in the problem of deducing the place of a single object, such as a satellite or asteroid, from a plate on which there are known stars. The complete investigation is promised in a forthcoming publication of the Lick Observatory, and it is perhaps a little premature to offer any criticisms. But from the account of the "necessary steps" given in the Bulletin it is rather surprising to us on this side of the Atlantic to see the process called a "simplification." Indeed, after reading the Bulletin more than once, I can only confess to some mystification. The questions suggested seem to me (but I may be quite wrong) to be these:—

(1) It is a considerable labour to transform the R.A. and Dec. of every star into standard co-ordinates: cannot we do this for the whole lot at once? or for the mean star?

(2) The distance from the mean star to the satellite or asteroid is small: will not approximate plate-constants suffice?

The second question is the more familiar, and practical answers to it, suitable to particular needs, have already been found by different workers. I may mention one illustration which happened to come within my personal experience. Prof. Schlesinger *makes* his mean comparison star coincide with his unknown object by an artificial process, viz. by weighting the measures of the individual comparison stars (within reasonable limits). He contends (and I think the position tenable) that to give a measure half weight is at least on no worse footing than omitting to make the measure entirely, which is clearly within his province. And by this process he simplifies his reductions considerably.

THE first question is new to me and seems worthy of the attention of practical astronomers. Suppose we have the R.A. and Dec. (say α and δ) of a number of stars and form the means of them, α_0 and δ_0; and suppose we form the standard co-ordinates ξ, η of the same stars and the means of these, ξ_0 and η_0. Then ξ_0 and η_0 will not in general be the standard co-ordinates corresponding to α_0 and δ_0; but it may be possible to deduce the corresponding standard co-ordinates by applying simple corrections to ξ_0 and η_0 and thus save the work on the individual stars. If we expand $\alpha-\alpha_0$ and $\delta-\delta_0$ in powers of $\xi-\xi_0$ and $\eta-\eta_0$, then there will (by definition) be no terms of an order lower than the second. Further, for a great many purposes, we may neglect all terms higher than the second; for the third order terms are in any case small, and stars on opposite sides of the mean star will tend to cancel out. Hence we are practically limited to the second order terms, which may be written:

$$\alpha-\alpha_0 = B(\xi-\xi_0)(\eta-\eta_0),$$
$$\delta-\delta_0 = C(\xi-\xi_0)^2,$$

where B and C depend on the declination of the plate centre. If the stars are equally distributed on both sides of either axis, the first of these will tend towards zero; and neither of them is difficult to calculate.

A PROCESS of the following kind would thus seem to be indicated:—Divide the stars available into (four?) groups, (east and west?, and north and south?). Form the mean α and δ for each group, and the mean measures (x and y) referred to the object as centre. Correct the means for the groups by formulæ similar to those just given. The α and δ of the object can then be inferred by two simple interpolations, since we have practically only linear formulæ to deal with. But though I tried to start with Prof. Perrine's premisses (or what I could gather of them) I do not find that I have represented any of his processes adequately or reached any of his conclusions. Perhaps this note may reach him in time to appeal for fuller explanation of his objects in the forthcoming publication.

'MATHEMATICAL DRAWING,' a book of 145 pages by Profs. Minchin and Dale (E. Arnold, 1906), contains some very pretty graphical constructions which are not so well known as they deserve to be. The early chapters are fascinating, and astronomers may find them of real practical value, *e. g.* the method for graphically converting a circular arc into a straight line, very approximately. "Let AP be the arc; produce its chord PA to Q so that $AQ = \frac{1}{2}AP$: with Q as centre and radius QP describe a circular arc cutting the tangent AE in Q; then arc AP = length AQ." Even for a quadrant this is true to less than 0·6 per cent.

Mr. Saunder informs me that he has had a prompt and very satisfactory response to his request (on the Moon's behalf) for a volunteer, to which publicity was given in these Notes last month. Although it is scarcely to be expected that two such volunteers should be forthcoming, it seems desirable just to mention the fact so as to guard against unnecessary duplication of labour. Moreover, if by good fortune there *should* be another in search of work, I learn from Mr. Saunder that there is plenty of work to be done. Undoubtedly the best course would be to enquire at the fountain-head; but for illustration I may quote at second-hand the following:—"There is a lot to be done by anyone with a micrometer in measuring the lengths of shadows thrown by lunar mountains on a tolerably level surface and recording the times at which the measures are made to the nearest minute. The existing measures of heights are very discordant, and a large part of the error is probably due to the difficulty of determining the Sun's altitude, which it has hitherto been necessary to find by measuring the distance of the formation from the Moon's terminator—a very vague limit. There are now so many points measured that far better determinations could be got by computing the altitude from the co-ordinates of the formation, and these would almost certainly give much more accordant results. Or anyone who preferred computing to observing might set to work in the same way on Schmidt's recorded measures. The necessary data of 2894 measures are given in the "Erläuterungsband" to his 'Charte der Gebirge des Mondes': the positions would have to be found from Franz's Catalogue or my own (Saunder's), and it would be necessary to compute a solar ephemeris, so that it would be a big bit of work."

It is stated in the Oxford newspapers, and therefore it must be true, that the present Vice-Chancellor of the University of Oxford publicly made an astronomical joke the other day. To make clear the point it must first be mentioned that there is an official of impressive presence who carries a mace (or some similar ornament) in front of the Vice-Chancellor whenever he walks abroad: conducts him from his "Lodgings" to each place of meeting, lies in wait for him while mere business is going on, and takes charge of him again when the more important march to another meeting becomes due. Now the present holder of this dignity (and he has held it many years, though Vice-Chancellors have come and gone) has the name of Moon. And the Vice-Chancellor is reported to have asked, when he saw a new summons imminent, the following conundrum: "Why is the Vice-Chancellor like the Tides?" If any reader is in difficulties about the answer he should write to "the Vice-Chancellor, Oxford," for a solution. It may be added that, when diplomatic enquiries were made of the official whose name has been mentioned, he was not prepared to deny the substantial accuracy of the journalistic narrative. He even went so far as to relate that, in years gone by, an irreverent generation of undergraduates had included him with another Vice-Chancellor in another jesting phrase. "There goes the Moon, and the Heavenly Body," they used to say. This was news to me, for it showed that the appellation "Heavenly Body" had been bestowed more than once. I had always heard of it in connection with a quite different person, who had an alternative name suggested by the difference between the sum of the three angles of a spherical triangle and two right angles.

But as regards Vice-Chancellors: it is distinctly pleasant to be able to realize that they are human after all. I will venture to emphasize the point by setting down a charming story of a Vice-Chancellor told by an eminent American astronomer. [I hope it is not robbing him, but the temptation to tell it is very strong, though I cannot reproduce it as he would tell it.] He was passing through England after an eclipse expedition and found that he could just catch a glimpse of Cambridge by a hurried journey thither from London on the only day left available, which happened, unfortunately, to be Sunday—unfortunately, because he had sent away his portmanteau before realizing the difficulty of getting shaved in London early on Sunday morning. After some vain attempts to find a barber, he was fain to give up the quest in order to catch his train; and so he presently found himself in a third-class carriage containing one other passenger. He was far from feeling comfortable or in the mood to approach strangers; and took out Baedeker to read something about Cambridge. The display of his researches produced a courteous enquiry from his companion in the compartment whether any assistance would be welcome; and he met with so much ready help and sympathy that he ultimately consulted so kind a guide on the subject of getting a shave. By this time he had incidentally explained who he was; but he was considerably and most agreeably surprised to get a simple invitation to the stranger's own house, whither he was driven in a dogcart: to be most kindly entertained in several ways; and ultimately to be sent up to the Observatory in the same dogcart that had conveyed him from the station. And on leaving he begged to know the name of his friend in need. "Well!" said the stranger, "I'm the Vice-Chancellor."

The example of the Vice-Chancellor of Oxford has quickly been followed by one of the Doctors of Civil Law. The skilful theft of the German shoemaker who dressed himself in officer's uniform and arrested the burgomaster of Koepenik in order to get hold of the funds of the municipal treasury, does not seem at first sight likely to give rise to an astronomical joke; nevertheless the following was extracted from it, and printed in the *Oxford Magazine* for November 14 last:—

Orbi et Urbi.

Sidera terrarumque olim Copernicus orbem
Circumagi medio sole minora dabat:
Cives subjectamque hodie Koepenicus urbem
Circumagi medio centurione docet.—D.C.L.

As Oxford has been mentioned, the excuse may serve to introduce the following verses, which are taken proximately from p. 178 of 'Curiosities of Clocks and Watches,' by E. J. Wood (Bentley, 1866); but are therein transcribed from the *Mirror* for 1841. I learn that there were in Oxford at that time clocks with but one hand: "certainly Carfax, Tom, and Queens' clocks had only one hand each," writes my informant, "and perhaps some others also. St. Giles's as surely had two, because I have them at this moment." Some of the lines would do no discredit to Tom Hood, but my reference says nothing at all of the author. Some of them, on the other hand, are rather halting, notably line 3 of the second verse: but I have verified my available reference.

Some Observations on Clocks and Watches; with an excellent Moral attached, and a Proposition for Lengthening Life Thirty-six Hundred Times.

How Oxford folk may measure time is hard to understand,
Since of their public clocks each one hath only got one hand:
But clocks have generally two, like any dame or dandy;
And, faith, to me the latter plan seems vastly much more handy.

How strange and complicate a frame a watch's or a clock's is:
Made up of little crooked wheels and crooked paradoxes!
"Time keepers" truly! they "catch'd a Tartar," neck and crop;
They KEEP time whilst they go with him, but cannot when they stop.

Some clocks and watches have THREE hands by which the time is reckon'd;
But strange! the THIRD in such a case is always called the SECOND:
Oh, can the bard explain a watch, nor can you understand one!
Which even when 'tis wholly new, is still a second hand one;

Nor in these little indices is much to make us ponder;
For what we mete with so much care, it were not wise to squander:
A minute, though a time minute, to waste was never so meant,
Nor think that even a moment is a matter of no moment.

The best of ways, the song declares, to lengthen out our days
Is stealing hours from night; but THAT is NOT the best of ways:
I have a plan to lengthen life of more peculiar powers—
To seize the minutes as they pass, and make the moments (H)ours.

Then take your time-meter with you wherever you may travel;
The hour for meat, or drink, or rest, or toil it will unravel:
'Tis meeter I can say from old experience; and 'tis sweeter,
For did the Muse not mete her verse, her verse would not be metre.

DOES ~~NOT CIRCULATE~~
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